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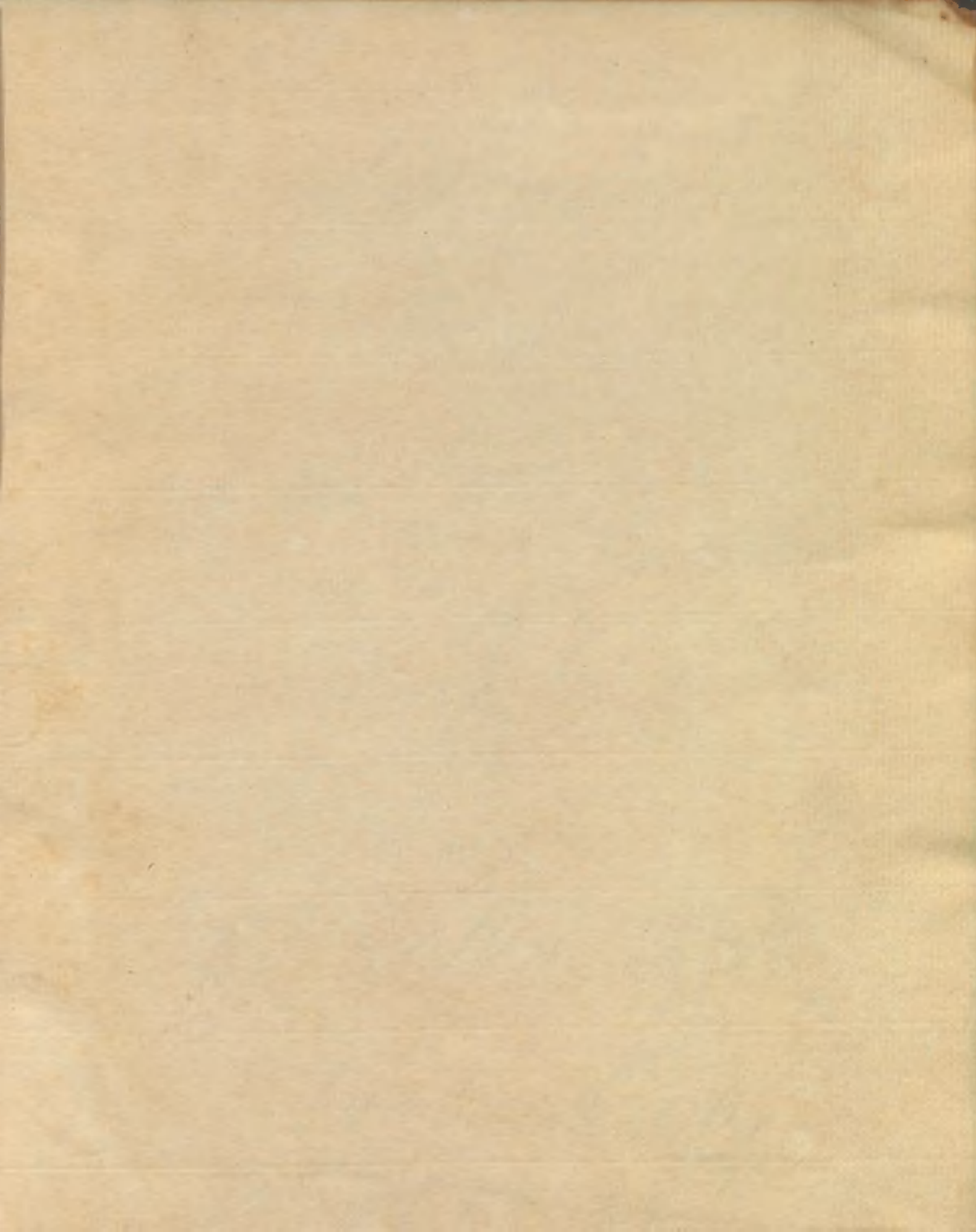






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Lectures

upon

the Practice

of  
Physick

by

W.<sup>m</sup> Cullen M.D.

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*History of Physic*

# History of Physic.

I shall now, Gentlemen, proceed to deliver a course of Lectures on the Practice of Physic; & previous to our entrance on the Subject, I shall prosecute the usual method of delivering a history of its progress, as far as we can with certainty collect from writers in the earliest ages. A History of this nature on a subject that is to be our Study must be equally amusing & instructive; and to observe the dawnings of Science, with the slow, but gradual, improvements of our Ancestors, in every successive age, must excite the Ambition of the Student, & prove a motive sufficient for an attempt of still further discoveries.

I was formerly, from similar considerations, induced to deliver (in my Institutions) an account of the Revolutions in Medicine, with the several Sects & Systems that prevailed among Physicians. — This I have reason to presume you are sufficiently acquainted with, and hence I shall



I shall leave the examination of this & merely confine myself to the state of practice; the particulars of which, as to its diversity in different ages, is a detail that may with more propriety be inserted at the end of our lectures.

### Of the State of Practice, as it occurs in Rude Ages.

In the early periods of society, when men have as yet attained to no pitch of civility & refinement, necessity, the mother of art, must first have given rise to the introduction of Physic. Men, in this uncultivated state, being moved by a sense of painful Impressions, are prompted by the dictates of natural instinct to make random efforts, & at first sensible exertions, for their relief. These random efforts, & the spontaneous cures effected by nature, have produced a species of art, which we denominate the Natural State of Physic.

On the peculiar character of Physic in this state I shall only make a few reflections.

Many are disposed to admit this natural state of Physic, as the origin & first dawning of the art, & have supposed it to proceed to the great lengths in the number & efficacy of their remedies. But this

This I am inclined to imagine is rather founded on a love of Antiquity, & Attachment to Empiricism, than on accurate Observation; for in this state no discernment of diseases & their different Symptoms can obtain, & of consequence no proper applications of remedies can be made: the Art altogether consists in the powers of the remedies employed, few of which have been found efficacious in the practice of modern Physicians.

From America the Materia Medica has been stored with numerous additions, all of which have in their turn been the objects of general admiration; but ~~unequal~~ unequal to the test of Experience, their credit has declined, & but one remains that merits the appellation of a specific remedy. — In the Highlands, a natural state of Physic is supposed to prevail, but its success depends merely on accidental circumstances; cures may be effected, but, that they are merely fortuitous, the following instance will be sufficient to prove. In the Highlands I accidentally met with a woman carrying a quantity of the common Digitalis or Fox glove, & upon enquiry discovered that she administered it for a Mania: the purpose of the woman could evidently be founded on no Experience, but was merely a random trial



trial without the least knowledge of the operation of the Medicine.

The natural state of Physic is without any accurate discernment of diseases, the acquisition of which will require collations of the observations of many different ages, & hence can only subsist in improved society. The merit of natural physic entirely consists in the knowledge of Emetics, but it is even deprived of this advantage; (but it is even deprived of this advantage) for, from experience of their inefficacy, they have recourse to Incantations, Talismans, & charms; & this strongly implies an ignorance of true remedies.

A second condition of Physic is, When the Arts are under the influence of the Ministers of Religion.

In the Ages of Superstition, when Mankind are subjected to the influence of a few artful men, learning, from political motives, will be confined to a small circle. As the Object of Priests is to attract the awe & admiration of Mankind, the means to effect this is to keep up the Ignorance & Barbarism of their Inferiors by preventing the general diffusion of Learning, & confining it merely to themselves.

Theo

The vulgar, in whom the smallest appearance of science creates the greatest admiration, being ignorant of its extent, immediately conceive it to be unlimited & esteem its professors to be supernatural & superior to the rest of mortals.

In this general Monopoly of science, Physic, so adapted from its effects to support the elevation of the priests, was of too interesting a nature to escape their attention; accordingly in Egypt it was inseparable from religion, & the directors of the sacred functions were superintendants of the sick & to these alone was permitted a liberty to practice. The circumstance of the Egyptian physicians practicing according to the books of Hermes gives us so contemptible an opinion of the state of physic among them that it supercedes all further enquiry.

Nearly under this form - Esculapius seems to have introduced it into Greece; & tho' Superstition was not so predominant among the Greeks as the Egyptians, yet the practice of the Priests exposed them to no small inconvenience. The Physicians being equally dispensers of Spiritual as well as bodily remedies had not sufficient leisure to visit their patients, & the patient was under a necessity



of resorting to the Temple, hence the Inspection of the priests must have been confined to chronic cases. Upon the whole it appears that Esculapius & his contemporaries possessed little more knowledge in Physics than is to be found in its natural state.

We can discover no traces of their proceeding upon any regular plan, & the reliance that was placed on superstitious remedies sufficiently evinces the deplorable state of their practice.

The trouble, & even the impossibility, of the patients resorting to the Temple, soon produced regular Physicians on a plan not greatly different from the practitioners in the present period. This we shall call

### The Artificial State of Physics.

At what period Physics began to be cultivated as an Art we are entirely ignorant, its first appearance in that form was some what previous to the time of Hippocrates. From the writings imputed to this celebrated personage we are enabled to form some judgement of the condition of Physics. In that period it was considerably advanced and purged from the obscurity with which it was formerly enveloped; here we find diseases accurately

(diseases) accurately distinguished & denominated, many aphorisms or general conclusions formed, many difficult operations in surgery described, & various remedies both simple & compounded discovered. Great regard is here paid to Exercise & Diet, & these subjects in particular are treated with a clearness & perspicuity that is astonishing for so early a period; and to all this there are many appearances of system to unite the several parts.

Notwithstanding this however the practices of Hippocrates is involved in great obscurity, chiefly arising from the Interpolations of Manuscripts & innovations of posterity; but still we have sufficient testimony from the writers of succeeding ages to denominate the merit of Hippocrates indisputable.

An important part of the character of this period is the first prosecution of Physics on a Dogmatical plan; but the state of Anatomy & every other part of Natural knowledge was so inaccurate that few attempts could be made towards the establishment of a regular plan of Dogmatism, whence appears the



the folly of the Moderns in quoting Hippocrates on Physiological subjects. In some respects indeed Theory was applied, a general system was aimed at & particulars connected, & on this plan general indications were formed. His practice was founded on the Maxim that diseases arose from redundancies, & hence a general Indication was formed to remove these by Evacuants. This was but a part of the general principle that prevailed of diseases being cured by their opposites, *contraria contrariis medentur*; and Hippocrates was guided by the *Vis Naturæ Medicatrix*, as a rule that was formed by which Art was directed and Nature every where followed.

The Dogmatists however have not on every occasion avoided the abuses of Theory or inconsistencies of System; a too great attachment to principles falsely founded may frequently lead to dangerous errors in practice; but this, tho' an object, is by no means in consequence of Dogmatism; with due attention it may be avoided in this, but is inevitable in the precarious & uncertain practice of Empiricism. (Sorry)

From the time of Hippocrates Physicians were Dogmatists, when two Men were much celebrated in the Annals of Antiquity. Erasistratus made many discoveries in Anatomy & considerably improved that Science till then in its infancy, he made several attempts in Physiology, but was insensible to the badness of their foundation or the uncertainty of their application in practice; he was led from Theory to neglect two of the most celebrated remedies, & bleeding & purging were abandoned from his practice; a natural consequence of an abuse of Theory which is apt to create an attachment or disregard to general remedies.

Theory in general, if not perfectly complete is attended with the inconvenience of giving misconception & timidity in practice; from this Theoretical timidity may probably be accounted for the neglect of purging in Erasistratus & his confining himself to a few Drugs.

The other, Hierapylus, was accurate in dissection; he cultivated the study of the pulse & other parts of Pathology; he was originally a Dogmatist, but distrustful his Dogmatic System was every where busy in the search of new remedies. A too intent.



intent investigation of Remedies is equally, with Theory, liable to abuse, & causes a neglect of System, & has an ultimate tendency to the prosecution of a random Empirical practice. In this case the propensity to Empiricism soon appeared by the establishment of that sect, & it might have been expected from the total Subversion of Theory and reasoning that a different state of Physics might be given by the Empirics. But these on the contrary have made not the least improvement in the history or discovery of diseases, neither have Remedies superior in efficacy been produced, or the use of those formerly employed better ascertained.

I shall now proceed to some reflections on the different forms the Practice of Physics has assumed.

I yesterday took notice of the ancient state of Physics as it occurs in every country in an early period of Society. It is little worthy of our attention indeed in this state, as in every uncivilized society we are certain of meeting with some traces of the pretended skill of Peasants in the cure of diseases. We mentioned it likewise in a more refined period where it was connected with Religion; but in this state as

is apparent from the Greeks & Egyptians its condition was little improved. Its being confounded with sacred matters is by no means peculiar to the Nations in which we have examined it, for in the western Empire the Druids were Priests, Lawyers, & Physicians. Among the former the sacred Tablets in the Temple of Esculapius were the sole rules of their practice & on these were founded clinical practitioners, from whence arose the Necessaries state of Physic.

It has been disputed whether this state of Physic was Dogmatic or Empiric, but Dr. Boerhaave has pretty clearly decided this point & has summed up a view of Hippocrates & the state of Physic at his period.

At present we shall investigate some circumstances that occasioned a further change in the practice of Physic, and here we shall advert to its introduction among the Romans by Aesculapius.

The Romans being perpetually engaged in wars, with their neighbours & employed in the civil arts, had originally no state of Physic. An attempt had been made to introduce Physic & Surgery by Andragaldus &c, but the Grecian art was too refined for



so rude a people as the Romans at that period—  
 accordingly it was found impracticable; but whether  
 the accounts of the banishment of the Grecian Arts  
 are to be depended upon is uncertain, the Romans  
 however conceived an aversion to it & opposed its  
 introduction. Sometime after, it was introduced by  
 Asclepiades from Greece, & he attended to the preja-  
 dices of the people, he avoided violent remedies &  
 followed the practice of Erasistratus. This method  
 was sufficiently agreeable to the Romans who  
 were now arrived at some degree of luxury & he  
 professed to cure, luto, colorito, ac facundè; his  
 ordinary remedies were Friction, Exercise, & Wine.  
 This is the practice that is most likely to prevail  
 among the luxurious part of Mankind, but it is  
 manifestly pernicious to the Art, for in such a state  
 placebos will abound & supersede the administra-  
 tion & discovery of more efficacious remedies.

Asclepiades made use of a specious Theory drawn  
 from the Philosophy of Lucretius & others of that age,  
 which was afterwards adopted by Thomson his  
 follower. Thomson considerably abridged the System  
 of his Master & established the Methodical sect. These

to their new Modelled System attached the mild and most practice of Hippocrates, & as theirs was the only reputable doct these principles universally prevailed till the time of Galen. Two men however of more liberal sentiments that flourished at this period must by no means be omitted. -

1. *Celsus*, sprung from a noble family & not destined by profession for a Physician, was free from attachment to the schools, & unbiassed with the principles of particular sects; he aimed at more general views, & is as perfect upon the whole as was compatible with such an early period of the Art.

2. *Aretaeus Cappadocius* was of a sect called the *Dogmatic*, and his practice appears to have been in the best condition, his applications proper without attachment to any particular mode of practice; unawed by Theory his practice was firm & bold, & every Remedy, Medical or Chirurgical, he employed.

## Galen.

We are now arrived at a period where we shall find Physics to be more steady & less hurt by the extravagance of sectaries. Galen pretended to follow



followed the practice of Hippocrates & accordingly made but few material alterations in the Art. His system he delivered free from the prejudices of any sect, and admitted every improvement experience afforded; sometimes he employed Empiricism, at others Dogmatism, and upon the whole placed Physics upon the best possible footing.

This System of Theory however was founded on too narrow a bottom & was a considerable impediment to practice, & the improvement of the latter kept no pace with the duration of the former which lasted for 1200 years. After him successively arose Philosophers of reputation to the number of 3 or 4 whose writings remain, but are the less valuable as the model of their practice was the same with the Galenical.

The age of Galen was followed by a decline of Literature & a total abolition of it in the Western Empire. In such a state of learning, Physics must necessarily have degenerated with the rest of the Arts & Sciences.

In the 9<sup>th</sup> Century after the Christian Era, the Arabians arose & embraced the Galenical system, & altho' a difference of Climate produced an alteration of Remedies

Remedies, yet the difference from the original system of Galen was immaterial. The European Physicians from the 12<sup>th</sup> to the 15<sup>th</sup> century again revived, but these were veritable imitators of the Arabians, fettered down to a system of Theory, their practice was equally inert & limited with their originals. At this period the state of the science was at the lowest ebb, & nothing but the writings of the Arabians were extant: at length the Books of Galen & Hippocrates were restored & the Arabians having in some things differed from the Greeks violent disquisitions arose & partizans appeared in favour of the different schools; hence arose the famous controversy about bloodletting which improved considerably the practice in the 16<sup>th</sup> century. At length however the party of the Greeks prevailed & the Arabians were gradually neglected notwithstanding the system of each was Galenical.

In the 14<sup>th</sup> century some doubts arose in opposition to the prevailing system. Argentarius criticized Galen & a considerable schism was in consequence formed in the school of Physic - a greater shock was however necessary to disgrace it, when Paracelsus by furious declamations against the ancients opposed these & every other leader in Physic, himself

intending



intending to become the sole Brackes. He soon acquired Partisans & formed a sect, & hence the division of Physicians into chemists & Galenists, & from his time till the middle of the 17<sup>th</sup> Century did Physicians adhere to one or other of these sects.

The Practice in each was extremely different; The chemists trusted solely to the power & efficacy of their remedies & degenerated into mere Empirics. The Galenists opposed the remedies of the former, & their practice upon the whole was superior, but still timid & irresolute.

In this state did they remain till some circumstances happily concurred to the destruction of these sects; a spirit of literature arose & doubts were conceived with respect to the validity of the Aristotelian system. Attempts by Verul and Galileo were made to introduce a new philosophy & a method of philosophizing by Experiments, which proved fatal to the Galenic & Aristotelian systems.

The Chemists, as favourable to Experimental Philosophy, were here cherished & admired, & in the 17<sup>th</sup> Century the System of Galen was in general disrepute. The Chemists accordingly possessed the Schools, and the downfall of Galen may be dated from

from the reversal of the decree against Antimony by the Faculty of Paris. The discovery too of the course of the chyle by Ascellius, followed by the still more important one of the circulation, subverted the doctrine of Galen on the liver & hastened his destruction.

The chemists now in possession of the schools became gradually more systematic, but could never be styled men of liberal & comprehensive views. Every thing was resolved into an Acid & an Alkali & then superseded the use of other remedies. The Cartesian philosophers took up the chemical doctrine of the six, and these were the first that introduced the notions of Lents & Vigidity.

The objects of the dogmatic system did not so much depend on the peculiarities of the Dogmas as their being limited in their views to the Animal system. They rejected remedies & excluded Experiments, by an attention to which the abuses on either side might have been corrected, and in these essentials every system since the age of Hippocrates were faulty.

The physicians universally both ancient & modern neglected the Organic system & preferred every thing to a temperies of the fluids. The Galenists were entirely

entirely negligent of this, and the Chemists rejected the doctrine of Plethora.

The Physicians however in the middle of the 17th century, when the doctrine of the circulation was fully established, were under a necessity of adopting the Organic System, & here the Mechanical & Cartesian philosophies were blended together & the system of Physic began gradually to enlarge, here however it received a check from the mistakes of Sylvius & Stmuller, till a fortunate accident gave a new turn to the practice of Physic. Sydenham, bred to Physic & free from the narrow views of sects, set out with the doctrine of Aurologia, & proposed to form a system from observation. He was more employed in observation than any one since the time of Hippocrates, & less governed by Theory than his predecessors, he rather sought for Theory to unite his observations under general heads than for facts to confirm his Theory. He had no general system of Pathology, but formed a particular one, at least for every genus of disease. Sydenham was followed by posterity, & at present every one is ready to reject Theory & adhere to good & accurate observations; & practitioners at present are Men that have the most extensive views of the Animal,



Animal Economy and who apply it with propriety, allowing observation to correct every part of their system.

From the example of Sydenham & the influence of the new method of Philosophizing, Physics has been cultivated without attachment to any sect, but it must however be allowed that the peculiar Doctrines of every system must have an influence on the practice of its followers.

I have now reduced the History of the Practice of Physics, so far as to give a notion of its general form in different ages.

The practice of Asclepiades was conformable to the luxurious age in which he lived, & instances of a similar practice occur in modern times; it was the Maxim of a Physician in London never to give pain to his patients, & we have an instance of his severely reprimanding a Surgeon of his own recommendation for the pain that ensued from an operation. But these measures, tho' pleasing to the patients & suited to the popularity of the Physician, are by no means adapted for the improvement of Physics.

The plan that Galen followed was certainly better, he was master of the facts, but unhappily founded his System on too narrow a basis; such a system falling into weak & ignorant hands must have  
an

evident tendency to enervate & corrupt the Practice of Physic. This accordingly happened to the Galenists, and in an after period the fury of Paracelsus was of use in reviving the Lethargy of Mankind. The innovations & almost total alterations of Paracelsus it is natural to suppose would meet with opposition. This accordingly produced two species of, & declares the Chemists & Galenists. The former confiding solely in the efficacy of their remedies were unacquainted with the nature of diseases & hence their practice was random & empirical. The practice of the Galenists upon the whole was superior, but their remedies were feeble & inert; being however better acquainted with diseases & regulated by the operations of nature they were probably more successful.

After the introduction of the new Philosophy & the many improvements in natural knowledge the practice of Physic was established on a better footing, and our present improvement consists in a better Theory applied with direction & occasionally corrected by observation.

It must however be observed that the partial bias of every System must have an influence on the practice of its followers, and on this point

it will be incumbent on me, shortly, to delineate the chief Systems in Europe that may influence the practice of Physic.

### 1. *Maahl.*

This System appeared towards the conclusion of last Century & made a considerable progress in many & several parts of Europe; it flourished with considerable reputation till within these 30. yrs, since which it has declined & given way to the introduction of other Systems. The peculiar Doctrine on which it is founded is the tendency of Nature to the cure of diseases.

The *Auroregaria* or true natural Medicine has always been a doctrine in Physic but was particularly extended by Maahl & his followers.

The Maahlians set out with the supposition that the functions of the body are under the Administration of the rational Soul & that the influence of this is not necessarily or mechanically directed to the state of the corporeal part, but is adapted for the preservation of the body in a manner agreeable to the Judgment of the Soul; on these principles they were led to deny the Mechanism of the body & to depreciate the study of Anatomy.

Contrary



\* Linker's *Conspicua Therapeutica*, where Baehl gives  
an account of his system & his progress in forming it;  
Baehl acknowledges that the share Mendel has in  
Aurogaria is very little concerned in his system,  
only that he proposes some metaphysical questions,  
& hence thought it necessary to explain himself. -

Contrary to this, some philosophers suppose all the motions & affections of the body are material, & depend on mechanical connection, & to this class do the Materialists universally refer all that disagree in their extending the powers of the soul. But between these extremes a Medium is to be observed, & such was the opinion of Hippocrates, <sup>Hallman</sup> Boerhaave, Haller, & Sydenham & with these I likewise coincide.

The general application however of the Autocatastasia is the more whatever explanation you adhere to, whether according to Hallman or Stahl, I merely disprove of the universality of the doctrine & its unlimited extent in practice. The followers of Stahl indeed have pushed the doctrine of nature to a length that their Master is far from authorizing. A passage from the original will show his opinion in a light not commonly viewed in.\*

This System has led its followers to study diseases with accuracy, but from the apprehension of interrupting the efforts of nature their practice was feeble & inert. They were averse to the administration of Opium & other powerful remedies & introduced others of the most superstitious nature.

W. H. Hall

They suppose that the most universal fault of the Animal Economy is its tendency to the plethoric state, & hence the influence of the Lungs is constantly directed to diminish it by an Evacuation. This fundamental position has been of the most pernicious tendency & has considerably affected the Medical practice. Dr. Haen has, with great Justice, attacked this notion in his *Tract. De Hemorrhoidis*. The Haemorrhoids in general have abused Theory & been affected with all the prejudices of a sect, and in every dispute with their Antagonists have discovered all the Acrimony of the most bigotted attachment to their own peculiar notions.

The taste for Natural Philosophy that prevailed in the last century had considerable influence on Physic. Mathematics were applied to the human body, and the explanation of the circulation by these became the fashionable mode of reasoning. The Cartesian Philosophy & the framing of hypotheses were exploded & the more accurate method of induction from Expts alone substituted. Sydenham was now in a special manner recommended & universally followed, till at length Boerhaave appeared and produced a System that



that is now the object of universal attention.

Boerhaave, with the advantages of a sound judgement & unbounded genius, was peculiarly fitted for the reformation of physic. He adopted the system of Boerhaave & culled every thing that was valuable from the Atomists & Cartesians. He gave a place to the systems of Hippocrates & Galen, & assimilated many facts & opinions, & withal gave an ingenious turn to the doctrines of the Methodic sect. He is called by Boerhaave the restorer of Hippocratic or eclectic medicine, & his system is complete with facts justly stated & conclusions accurately drawn. He embraced every collateral science, was a Metaphysician, a natural philosopher, a chemist & Anatomist. Intent on the improvement of natural philosophy from reports on chemical bodies, he ascertained a variety of facts & published a system of Physic as accurate as the state of knowledge at that period would admit of. I admire the genius of Boerhaave & the extent of his system, but, after 50 years of this enquiring age, I suppose no detection, or amendment, has been made; would

would be a peculiar instance of partiality and folly.

His system of Boerhaave is in many respects imperfect, defective, & mistaken; his doctrine of the simple fibres is in many respects incomplete & inaccurate, nor does he keep in view or even comprehend in his system the affections of moving fibres. His doctrine of Acid is inconsistent with the rest of his chemical doctrine; and his notions of Alkali are equally liable to the same objections. In latter times the subject has been elucidated by the reports of Pringle & Macbride; who, by experiments on the Scurvy, have established the matter on a new & more advantageous footing. His doctrine of Acidity & Scurvy was equally incorrect in the system of Boerhaave as it was when first started by the chemists & Cartesianists; & the doctrine, the leading maxim in the Pathology of compound diseases, was still wanting to be explained.

In general Pathology, when he is treating of the effects of Circulation, he is extremely imperfect, & pays little attention to the moving powers

powers of our system; thro' out the whole, he is elegantly systematic, but there are few parts where addition & correction are not absolutely requisite.

He stated the affections of the Solids as well as of the fluids & connected the several parts of the Economy in a manner superior to any of his predecessors; a proper view however of the moving powers was still wanting, but he seemed insensible of the influence of the affections of the moving powers in explaining the several Phenomena of our system independent of the changes of matter.

Van Helmont had formerly attended to this subject, but Willis went farther & applied himself to the Pathology of the Nervous System; his Theory however was by no means correct, and Baglivi who succeeded him made some attempts which promised much but were never brought to perfection from the unfortunate death of the Author. Hoffman again revived the subject & has delivered in his Chemistry the general doctrine of it, aiming at an application of it in every part of his System.

Ch.



In the Institutions I observed the Animal Economy was made up of three Systems, the Nervous, Chemical, & Hydraulic - till the distance of a Century the Galenists & Chemists had attended to little more than the Chemical System, but Hoffman, to the Tempories of Galen, & the Hydraulic Systems of Boerhaave, & Bellini, has added the Nervous System, which renders it complete with respect to the parts of which it can consist.

The subject however of the Nervous System is difficult & obscure, & Hoffman with all his knowledge is not clear & systematic - his language is metaphysical & obscure, & he has every where intermixed a chemical theory that is far from correct.

The attention of Physicians was at first confined to the condition of the fluids, but afterwards the plan was enlarged & the affections of the Hydraulic System were closely studied. To these views were the followers of Boerhaave confined, and Hoffman in the important parts of his System has had few intelligent followers. The Nervous System however, notwithstanding the neglect of

of Physicians, & the natural obscurity of the Subject; is yet of extensive application, and the most deserving of any to be the basis of a practical system. I regret that I am unable to discover a general System of Nervous pathology, for nothing can be more obvious than the necessity of a System on that subject; the affections of the primary functions must be accurately investigated or all the subordinate ones must be involved in obscurity. The task I am sensible is arduous & difficult, but I hope to demonstrate that a prosecution of this plan will be serviceable in the explanation of diseases, and in effecting their cure.

(The Origin & progress of our Subject being now ascertained we shall proceed to examine the peculiar method of practice we intend to prosecute; some idea of this has been given when we mentioned wherein the improvement of the present state of Physic consisted).

At present we have a better Theory with a more cautious employment of it, joined to a constant attention to Observation & Experience. To explain ourselves more fully—Our Theory is better because our Anatomy is more complete & correct. The improvement in our Chemistry is considerable, and its

its parts more systematically disposed, & hence of an more general application & utility.

3. As our acquaintance with the history of Nature & Philosophy increases, our views become gradually enlarged, and our knowledge of physics & perfection is improving; for previous to any progress in this latter art, a considerable advancement of the former must necessarily precede, and thus the object of our study is dependant on the state of perfection the other parts of Science have attained.

Our Accuracy in Observation & Description has led us to many useful conclusions, but our theory is still defective & imperfect; many of our physiological reasonings are precarious & uncertain, & nothing could induce me to boast of the present state of Theory but the caution of practitioners in the use of it.

Theory in the hands of the ignorant and weak, may be productive of consequences the most pernicious to practice; but the Man of Science will always distinguish between what is to be repeated as fallacious & uncertain, & what is of good and useful application.

The greatest practitioners, Sydenham not excepted, were universally Theorists, & the theories of each were



were different in various degrees of perfection, & nothing but the most accurate discernment could have enabled them to conduct their practice with safety & success.

In the present age Dogmatism has a better chance of succeeding than in any former period; at present we are jealous of Theory & at all times ready to appeal to & correct it by experience, and hence our attachment to Theory more attentively engages us in collections & improvements of observations. Mr. Quanaix pays the highest compliment to the Memory of Boerhaave in calling him the father of collective Medicine. More facts were embraced & observations collected by this celebrated author than by any of his predecessors. Theory in such skillful hands may be safely attempted, because there are provided with means to correct its excess, and to adapt it with discretion to the facts.

With our modern philosophers, Experience is the Test - that is constantly appealed to; & by that standard are our reasonings corrected; we are so strict in the observance of this that our more judicious Scientists will scarce admit a Theory to be conclusive which they cannot establish as a fact.

In the cultivation of Physics a Situation of importance

ance' arises, whether the Study should be prosecuted on a Dogmatic or Empirical plan, or whether from the reason or result of Experience?

The Schools are universally Dogmatic; but the greatest part of Practitioners are professed Empirics. Students upon their entrance on the Study are influenced by the latter, & are thence rendered indeterminate &averse to System. This is productive of a most pernicious effect; for the Student is prejudiced against a measure that at necessity he is obliged to pursue; as, from a neglect of what they call Theory, facts of the utmost importance are slighted and passed over. It would be here too great an intrusion on our time to afford the subject a more full & ample discussion; but, as I have formerly treated of it in my Institutions, I would again recommend it to your perusal.

It is however by no means necessary to give an absolute determination in favour of either; both are exposed to fallacy, & each is separately insufficient in the present state of Science. Every one must admit the necessity of Experience; yet Experience has furnished us with more erroneous facts than Theory with false reasonings. And on the other hand no one would plead for the sole use of Theory & description of Experience;

a tenacious adherence to System has often corrupted our Art, yet no one can refuse its utility to a certain extent. Every one can perceive the Errors of Theory, but even those who most violently oppose it are obliged to have recourse to it, & Theory is ever introduced, even in practice the most Empiricall. For a clear & comprehensive view of the subject it is proper that the doctrine should be further considered, & the limits of reasoning & Experience be precisely ascertained.

I shall reduce my Arguments to three.

I. Reasoning in Physic is unavoidable, & to render it safe it is necessary to cultivate Theory in its full extent.

II. The Study of Physic on a Dogmatick plan has given us the facts we have already acquired & is the most probable means of acquiring those facts on which we would found an Empiric plan.

III. There is no foundation for the Study of Physic on an Empiric plan. )

I. Reasoning in Physic is unavoidable, & to render it safe it is necessary to cultivate Theory in its full extent.

In Human nature there is a strong propensity



to seek for causes, & Mankind endeavour to explain every thing as a cause & effect; this tendency I imagine is incoercible & inherent in our natures. Sceptics may declaim & even demonstrate the fallacy of human reason, but their efforts are unable to destroy this favourite propensity; its indulgence is productive of good, it improves our reasoning, exercises our faculties, & directs the attention to every consideration that may influence our judgment.

Were a physician to attempt reasoning with a Lawyer on the subject of his Profession, he must be found extremely deficient, he would incur the contempt & ridicule of his antagonist. The same consequence would ensue were the lawyer to invade the province of the physician, and the sole means of conciliating the matter would be to engage each in the study of the different subjects in their full extent.

Such is the rage for causes, that instances are not unknown in which their investigation is totally neglected. Every practitioner must have perceived this tendency in his patients, & Physicians themselves, however they may detract from Helmont, Paracelsus, & Astruc, will yet upon

examination be found to be Theoretical; hence in their practice, this man is plethoric bleeding, must cure, the stomach of another is full, let an Emetic be administered; and thus they proceed insensibly in a plan of Theory, which from a want of attention to the system is generally of the most pernicious nature. In the practice of such I have frequently been witness to the gross errors, corpulency has been mistaken for Plethora, a Sympathic affection of the Stomach for impurity, & a slight cutaneous eruption for an Acrimony in the blood. Every Empiric therefore is in some degree a reasoner, & as the abuse is great & the propensity incorrigible, the only remedy to be provided is for men to engage in the full extent of the Study of Theory.

Every part of the Body we know to be connected with the rest, & the whole taken together forms a System, the application of a part to the whole will assist us in the discovery of fallacy, & on the contrary, the application of the whole to the several parts will enable us to ascertain the limits of the profession. No man will restrain his own reasonings unless he is much exercised

exercised in the fallacies of others, and hence the service of Scepticism. Theory in general is uncertain, & the abuse of it can only be corrected by studying it in its full extent, & on a dogmatic plan.

"II. The study of Physics on a dogmatic plan has given us the facts we have already acquired, and is the most probable means of acquiring those facts on which we would form an Empiric plan.

That useful facts have occurred from fortuitous circumstances, yet these would never have been prescribed for application to Physick but for the systems & enquiries of the Dogmatists.

The ancient & most modern labours of the Empirics are sunk in oblivion, & it is entirely to the systems of Dogmatick that we are indebted for our improvement in Physick; but that this study is essentially requisite for a collation & discovery of facts may not be as obvious & will require a further discussion.

The Empirics are supposed to be acquainted with a variety of remedies adapted to particular diseases, but in those cases where

diseases



diseases resemble each other their remedies, are the same, but it is an essential circumstance to Empiricism that diseases should be accurately discriminated, & this distinction to be correct, must be a reduction of their particulars to General Species, & a Nosologia methodica is necessary for the foundation of an Empirical practice; but this so essential a point to Empiricism was totally neglected & its subsequent production was altogether the effect of the study of Dogmatism. The first attempt towards an Execution of this plan commenced about 40 years ago by Mr Sauvages, & this has been the only attempt to ascertain the species of diseases.

Every one however the least conversant with System or acquainted with the laws of method must observe the inaccuracy of his classification. I shall shortly deliver to you some plans of what has been attempted on the subject, but at present they are all incomplete & imperfect.

Hence then the chief & only foundation of an Empirical practice was wanting; on this side it may be alleged that they have facts sufficient

ficient for a foundation, tho' they are not sys-  
 tematically arranged, but that these are suf-  
 ficient for the demands of practice. Absolutely  
 deny; they are neither united into a system,  
 nor are the particulars well known or accu-  
 rately defined; they are neither united into a  
 system, nor are the particulars well known  
 or accurately defined. It is evident from na-  
 tural history that the only accurate mode of  
 definition is by Genera & Species - the objects  
 of that science have remained the same within  
 these 100 years unarranged, and we see that  
 the succeeding attempts of Systematics have  
 brought out & defined the vast variety of par-  
 ticulars which Linnaeus has given. So in the  
 objects of Zoology if the method of classifica-  
 tion cannot be properly performed we may  
 be certain that the particulars of which it is  
 formed are not accurate or complete, nor is  
 it possible they should be we are further  
 advanced in system; hence the imperfections  
 of Sauvages require to be supplied by further  
 observations, & the collations of these can only  
 be made by our further advances in system.

Dissections

Dissections, by exposing the parts to our view, show us the changes to which external appearances are owing; Sauvages, tho' he refuses the internal structure of the body to be necessary to the classing of diseases, yet tacitly employs it; and it is evident that dissection, by demonstrating the changes in the system on which the various affections depend, must lead us to a proper limitation of Genera & Species.

This like many other attempts on System will lead us to an accurate observation of particulars. Our judgement of morbid bodies will depend on our knowledge of the healthful state of the parts, & our Dissections will be more accurate in proportion to our progress in Anatomy, and hence the necessity of Anatomy to Zoology. Anatomy owes its progress to our Study of the use of the parts, and the Study of the use of the parts is necessary to the study of the whole, and an Attention to Physiology is as necessary to direct our knowledge in Anatomy as much as the latter is essential to our improvement in the former.



To determine therefore decisively between the advantages of reason & experience is unnecessary; either of them separately considered have contributed little to the improvement of our Art, and it is only by a judicious union of the two that we can ever hope to establish a regular foundation for a Practice of Physic. The distribution of diseases is guided by morbid dissections, and our accuracy in these depends on our improvement in Physiology &c; it appears therefore that our knowledge of diseases is more effectually cultivated by a system of Dogmatism, and from the history of Physic it is evident that by the study of Proximate Causes we are the most readily conducted to the nature of diseases.

To proper foundation therefore for an Empirical practice existing, & reasoning being inseparable from the constitution of man, it necessarily a system of Dogmatism must prevail, & be the only foundation for a regular state of Practice. It is the study of System that leads us to the investigation of facts; an illustration of this from Natural History has been given, & if

it is equally efficacious in the objects of Nosology.

Hypotheses, when they enter into our reasonings, are justly to be condemned, but they are highly useful, as they are only revolved in the imagination of a Philosopher. The least acquaintance with Philosophy will sufficiently convince that we are indebted to Hypotheses for more than 100 Observations and Experiments, where for once we are indebted to Empiricism. The System of the universe was explained by Sir Isaac on the foundation of an hypothesis, & hence the utility of these in the acquisition of facts for the foundation of an Empirical plan.

In any view they are of the utmost importance, for what are usually denominated facts are more frequently the inferences of reason than observations of simple sense. Single solitary facts are only useful as they are generalised and compared, and from these an induction is formed, in which operation to be perfectly correct the utmost accuracy & extent of human Judgement is requisite.

By reasoning are the most of our Facts ascertained, as examples of which we shall specify —

1. Those

I. Those that relate to the Study of the remote causes of diseases, and

II. Those that relate to the Study of the Remedies.

I. If the observation of the remote causes is useful in (Physic) & necessary to establish the proper distinction between the several diseases, then Dogmatism being the only foundation of System must be necessary to ascertain the species of remote causes. When the body is exposed to the action of many different powers we are uncertain how many of these are concerned in forming the disease, nor can this be ascertained till the Philosophy of those powers are accurately investigated. We know that their effects are varied by the state of the body & hence the Philosophy of powers in general & particularly Physiology & Pathology must be studied in order to determine the doctrine of Remote causes.

II. To judge of the effects of remedies a Dogmatic System is especially necessary. As the consideration of Remedies often leads to a distinction of diseases this is a head to which most of the Empirical facts are to be referred, and



III<sup>x</sup> Any attempts to teach Physics on an Empiri-  
cist plan have been fruitless and even prejudicial.

and those with respect to remedies whose operation is involved in obscurity, even that boasted Medicine of the Empirics, the Peruvian Bark, is by no means to be esteemed as an invariable specific, on the contrary its effects in many instances where its efficacy was alledged, have been justly disputed.

The effects of remedies can therefore be only ascertained by the Cultivation of a Dogmatic System, as those & the generality of facts are rather to be referred to an internal operation of reason than to be esteemed as the product of simple perception.

\* III. There is no foundation for the Study of Physic on an Empiric Plan.

Sydenham seems to have attempted a practice on an Empirical plan, & as far as he has proceeded is truly useful; his System however is by no means answerable to the demands of Practice, & if this at present was our only guide the cure of diseases would stand on a very narrow basis.

It can however by no means be admitted that Sydenham proceeded on a plan purely Empirical; in his larger work his practical conclusions

one are entirely Dogmatical. Other attempts, have been made to establish a practice system upon this footing. Dr Shaw and Licutaud have each taken up the subject, but the book of the former is frivolous & forgot - the latter has been better received.

The work of Licutaud is an attempt to establish an Empirical practice; but his system is altogether insufficient for the purpose; this as well as every other attempt of that kind must be imperfect in design & execution, & extremely pernicious to the constitution of Physic.

In his Book some judicious observations & reflections are to be found, & on the other hand many errors & inaccuracies. I propose to consider it as a system & guide to practice; & in this view it is altogether insufficient; he has no where ascertained the species or even genera of diseases, & a defect in so essential a circumstance is incompatible with an accurate & intelligent practice. He has attempted an Historia Morbi, or a list of circumstances commonly referred to one title or what are denominated the genera of diseases; but he  
has



has not denominated the succession of diseases to distinct species, as an example of which I refer to his Article of *Hypochondriasis*. Here we find an undigested mass of symptoms from which the most precise & undetermined notions can only be obtained. He has afterwards turned a genus of a single symptom, & under that title has given over the cases under which such symptoms appear, hence in his history of it he gives symptoms of many different diseases, as in *Dolores*, *Siccitas*, *Alvus reticula* &c.

His dissections are merely a promiscuous enumeration without any distinction of causes, <sup>and effects</sup>. This part of his work is in some measure useless as he has neither connected them with a particular concurrence of symptoms, or employed them in ascertaining the proximate causes of diseases.

The system of Licutaud upon the whole is extremely insufficient for the purpose & has procured a most indeterminate method of cure. His remedies are collections of every thing that has been given in all the species of any disorder, which he so confusedly heaps together under a generic name. In his account of the *Ystheria*, having described

described the disease & given us the dissection, he says down this general remark, "while the causes of diseases are involved in obscurity it will be impossible for us to attempt any method of cure, & we must abstain from our remedies & be merely solicitous about a suit-  
able regimen." If these are the fruits of experience, they are truly deplorable, & little have physicians to boast of, & only these are the results of the labours & study of <sup>our ancestors</sup> so many generations.

I went back over his list of medicines & re-  
viewed what he calls his established practice, wherein he displays such inattention to method that an Apothecaries Apprentice of 12 months standing could be able to give a better arrangement. For instance, a few will be sufficient. Pearl Beeth & Gum Tragacanth, Sars & Tan water, &c. Read over any of his cures & an experienced practitioner will often be unable to say for what they are prescribed.

As I observed in some parts of his work is extremely inconsistent. In his Præface he has added a Synopsis of Medicines & has endeavoured to explain the operations of their operation, but to what purpose does he admit Mercutio's history.

System that forms no indications? The reason is evident, throughout the whole, he proceeds upon dogmatical facts without which no system of practice can be formed.

Such then is the present state of Physics, such is the practice of Siccardi & such the boasted foundation of Empirical facts. From their inattention to System Experience is little advantageous & it is only from an Union of the two that a proper foundation of practice can be made.

In the 3 Arguments I introduced I have endeavored to obviate the opinions now commonly maintained, of the superfluity of Theory, with respect to the first I shall give one further illustration. In the last century when the dispute subsisted concerning the comparative excellence of the Ancients & Moderns, Mr. Fontenelle, who added the side of the Moderns, made use of this Argument That the more numerous are the reasonings we have rejected the more have we arrived at truth, & hence in many many conjectures have been rejected & therefore our approaches must be nearer to truth.

As to our 2<sup>d</sup> Argument I may appeal to the Experience

persons of every man for its being well founded.  
No one can have escaped the observation that  
books can be easily marked or collected, as an  
Hypothetic system is provided for their appli-  
cation.

On the 3<sup>d</sup> I was employed in a disagreeable task,  
a criticism of the System of Lavoisier; it was how-  
ever absolutely necessary, as all attempts to  
establish Physics on an Empirical plan are vain,  
insufficient, & trifling. An Argument advanced  
against Lavoisier & commonly repeated, I went  
on to answer to show, viz: That Physicists at the  
same time & in different ages, & moreover they  
may have differed in theory & in their mode  
of reasoning, have come to the same conclu-  
sions, & adhered to the established mode of Prac-  
tice; hence the trouble of Theory & the investiga-  
tion of reasons <sup>are</sup> cause unnecessary and in-  
dignities.

But to fret is consequently the conclusion is  
entirely destitute of foundation. At no time  
& in no country a particular system generally  
prevails, but it varies. There are those that  
think for themselves; it is by no means an  
ordinary that people should tread in the steps



of their master and be consonant in their  
 mode of practice. Some little time ago the  
 Boerhaavian practice was universal in this  
 country, & the Physicians that were capable  
 of Theory were more imitators, & thus the same  
 mode of practice was cultivated by all; &  
 arose either from some Theory universally  
 pursued, or a more Empirical practice with  
 equal principles in those that were incapable  
 at home. The models of some indeed may  
 produce a harmony in consultation, but  
 conformity when left to himself will be shaped  
 by his own peculiar notions. Every sensible  
 Physician must observe the difference of prac-  
 tice among the Galenists, Chemists, & Paracelsians,  
 & the mode of proceeding with his different  
 and very different subjects suitable to the situation  
 in their principles. Diseases are the same  
 in every age & period, Nature & Experience  
 have occasioned the formation of general  
 rules in practice that are not to be contra-  
 dicted, but in instances of a less uniform na-  
 ture practice will be directed by Theory.

The conclusion is unavoidable that at times  
 Theory

Theory has been truly pernicious; no one can reflect without horror on the death of Van Helmont who died of a Pleurisy from a neglect of Evacuation. This treatment of himself was owing to his principles, & he fell a sacrifice to his doctrine. The practice of every age must be influenced by Theory, & if the opinion of the Empirics is significant of any thing it must be the rejecting of Criminate causes & merely attending to the distinction of viscous & the common practice that prevails. To this plan Practitioners in common adhere, deficient in system & destitute of the tradition necessary to the Art, they confine themselves to these limited views & proceed on what they generally denominate the established practice. But a practice so established can only be established by Argualism, for Experience is frequently erroneous & ever requires that correction that Theory alone is able to afford it.

Having thus argued for the cultivation of such a Plan, I must confess it is pursued with difficulty, & exposed to fallacy, from which  
(consideration)

consideration I must earnestly recommend an examination of every piece of reasoning not obvious or simple. In attempting a system it is difficult to avoid subtle & intricate reasonings, and hence as a consequence of these the inferences may be hazardous & fallacious; as there must sometimes occur, caution shall be given previous to their delivery, & I shall endeavour by the suggestion of hints to divert any doubt that may arise. Guided thro' the whole by such cautious views I hope to avoid the bad consequences of system; On Theory no further reliance shall be placed than necessity requires; every fact shall be duly communicated & no conclusion shall be formed that any rule in Physics can be axiomatic, and by giving facts on a dogmatic plan I shall give them with the advantage of being generalized.

This is my plan of practice & what is implied by a dogmatic plan you must by this time be perfectly acquainted with. A plan of Dogmatism supposes,

1. From the study of Mechanical & chemical Philosophy you know the several powers acting on the body.

2. That

2. That from Anatomy you are acquainted with the structures of the body itself.

3. From Physiology you know the laws by which the Animal Economy is governed.

4. From Pathology you know the deviations from Health.

5. From a comparison of Physiology & Pathology you know the changes that are made to restore the body to it's healthy state, or to know the indications of the *Methodus Medendi*. and

Lastly. To know the several powers that restore health.

I shall now proceed to give you my particular plan.

I shall not treat in Pathology of simple diseases, but shall mention diseases as they actually occur in nature, & formed by a concurrence of these called simple diseases. I shall consider them as a concurrent concurrence of symptoms or according to a *nosologia Methodica*. for this purpose I shall put into your hands the Systems of Sauvages, Boerhaave & Linnaeus, & to these subjoin a different arrangement of my own.



In treating of every particular disease, under each title; I shall first deliver the Phenomena in currency or succession that belong to a genus. 2<sup>d</sup> I shall endeavour to investigate the proximate causes, & these on some occasions I shall be able to report to explain simply & clearly, & in others so it only in part, in such however I shall point out the proper train in which they are to be pursued, and apply a method of cure.

In some cases I shall be unable to rise above a pure Empiricism, & on the subject of Proximate causes we shall discover the utmost candour with respect to the opinions of others & the strength of our own reasonings. I shall endeavour to render them safe by proceeding on the principle of finding out what truly happens in the System, not shall I attempt any inferences from reason that are destitute of foundation in fact. I shall follow the Pathology of Sydenham & give a specimen of Dogmatism unknown in the Schools of Physicians; & being thus ascertained the Proximate causes from these & the Phenomena of diseases, I shall attempt to limit the sphere & division of species. I shall endeavour to render the remote causes, not

not from conjecture but as guided by observation  
on & experience. Here I shall endeavour to  
reconcile with those facts of the Proximate  
Cause previously delineated.

From the Symptoms of diseases I shall give  
a rational System of Prognostics, and lastly  
the *Methodus Medendi*; the plan of which  
shall be formed on the Doctrine of Proximate  
Causes. My practice will be founded on Indi-  
cations, but I shall be ever cautious in avoid-  
ing subtle reasonings but rather examine the  
State of the Body as a fact than infer it by  
reason. I shall take notice of every mode of  
practice & every method proper to be used, &  
after all if you reject my system of Dogmatism  
you will have every resource: Impericisms has  
proposed. Every thing deserving of attention  
shall be mentioned, & I hope to comprehend all  
that is material & most likely to occur in your  
study or practice. —

As to the condition of your Studies, very lit-  
tle reading of Books can at any rate be ad-  
visable; your time will by no means be  
lost if it be many if you are not truly profited

for the purpose of Nations at Large. With regard to the Principles of Physics, facts may be delivered simply, or such as are already known in order to be applied; or they may be delivered in a different form & improvements made in their arrangement & new views may be proposed, hints suggested, & new matter communicated.

As to the first Plan, to men of Science it may be of the greatest utility, but to others, whose views must be directed, a method must be laid down as a Model for the future prosecution of their studies. I must confess I should have but a mean Idea of the diligence & genius of a Professor that should pursue the former plan, as for any person who has new matter to give it is impossible he can follow the train of any particular Author nor of consequence can direct you to Books.

To those Gentlemen however that have made some progress in the study I would recommend the three most considerable Systems that have appeared on the subject, viz. of Maclaurin, Hutton,

to Boerhaave with these I should wish  
 you to be acquainted. In the Genera that  
 I have compiled of the several Systematics,  
 I have referred the Synonyms to these Systems.  
 Stahl indeed has published no System; but  
 his Pupils, Junker, Alberti, &c. have delivered  
 the doctrines of their Master, to the first  
 who is most in repute I have referred. The  
 only other Books I can venture to recommend  
 are Sydenham & the Nosologia Methodica  
 at Leipsic. Many others might likewise  
 be read, but as yet you are not prepared  
 for their perusal. The particulars relative  
 to these I shall reserve to the end of my  
 course, when I propose to deliver a distinct  
 Criticism on Practical Writers.

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I shall now enter particularly on the subject of Practice. The Practice of Physic has two Objects in view.

I. The Preservation of Health.

II. The Cure of Diseases.

The first (Hygieina) is a part of the Institutions of Medicine, but there we omitted to deliver it, for a reason that will hold equally with respect to our practice, because we have no direct means of preserving health. Our Hygieina entirely turns on avoiding diseases, the prevention of which is founded on the knowledge of the Predisposing and Predisponent causes which are parts of our Pathology, and so far as they are delivered their application is so easy as to supersede the Hygieina.

One application of it however is necessary, with respect to particular diseases, which require certain Prophylactics, and in this course they will be mentioned as a part of what we are to deliver.

The other part is the Cure of Diseases. In entering upon this I have to say that

notwithstanding the Arguments I have urged in favour of Theory & Dogmatism it is much to be wished that the greatest part of our Studies be founded on Experience.

The first object of our study is an accurate distinction of diseases. The vulgar, as well as the young & inexperienced Students, are apt to imagine that diseases we know by their names, & that is necessary for the Physician is a knowledge of the remedy connected with the name of the disease: it would be injurious to suppose that you, Gentlemen, can have entertained so erraneous a doctrine; but it is necessary you should understand that diseases ought to be more accurately distinguished. If I meet with a disorder I am unacquainted with I am of course at a loss for a remedy, my only resource is to consult the observations of Authors. If an analogous case should occur & a remedy be proposed, from an experience of its success, I immediately administer it to my patient, but find it is entirely ineffectual. In this case as I have no doubt of the accuracy of the Author or of the goodness of the remedy, I must inevitably conclude I have mistaken the disease.

Every one that has been engaged in practice, must have met with such instances, & this is sufficient to show that more observations are found to be useless. The method of proceeding is first to give the remedy applied to the name of the disease, & then attempt a definition, but by no means exact or complete, it is much to be regretted that in defining diseases we have been extremely defective. The only attempts have been made by the Dogmatists, but these have defined them from their Theories, & hence must have been liable to fallacy.

Sauvages in his *Prolegomena* has given no less than ten definitions of Pleurisy, but all of them are insignificant, and many systematic writers being sensible of the impropriety of this have attempted a *Historia Morbi* or a full enumeration of every symptom belonging to a disease, this however is far from being exact or complete or even applicable to our purpose, for every practitioner will find more symptoms in the description than occur in his patient, & sometimes on the contrary more symptoms will appear in the patient than are.

are to be met with in the description, nor are the concurrence or series of symptoms any where marked as separately occurring.

It has long been perceived that descriptions, however comprehensive, are by no means sufficient; we want what are usually denominated Pathognomonics, i.e. from the whole list of symptoms that are attendant on the disease, we select those few that are constantly invariable & peculiar to the species of any disease. This we have long wished for, but the marking of Pathognomonics has hitherto remained unattempted.

Stahl, Boerhaave, & Hoffmann, only admit of the genera of diseases, but that period the state of science was so low that this was the utmost they could attempt; the treatment; however of diseases under general heads is by no means sufficient, the cause must be ascertained, which these Systematics left to the application of their Theories.

A more accurate distinction of diseases might have been expected from the Empirics than the Dogmatists, but in this we have been



been disappointed. The Empirics have trusted to their analogy & distinguished them in a general way, nor have their successors in modern times more happily succeeded.

I have already taken notice of the imperfections of Lieutaud, his view in his Synopsis was to place practice on a better footing, by avoiding the discussion of Proximate causes & ascertaining facts. He has failed in his distinction of Diseases. In the Hypochondriasis, 100 symptoms are enumerated which have all appeared in the disorder & hence are termed Hypochondriacal; but it is sufficiently obvious that the whole of these symptoms have never in one Patient appeared & we are at a loss to distinguish in what particular concur, how they constantly appear, & how they are separated or combined. In his method of Cure he sets out on a Theoretical plan, & what are the Pathognomonic or Essential Symptoms, & what are the accidental & undistinguished, he does not point out. Upon the whole it is impossible that the Empirical Practice can ever be or be worse,

worse footing than it is with such a list of undistinguished symptoms unless indeed we are ceapt with such a mass of undigested remedies.

We are not however wholly dependant on Siculand; among the English, Sydenham, Meadon, Sturtham, Sydenham, & Pringle, have given us descriptions of diseases, accurately distinguished in their parts, but it is only in a few diseases that these writers have afforded us assistance, & our system can never be complete till every observation is collated & compared with others.

Dr Pringle has given an account of the Marsh Fever, but this would have been of little importance, if he had not compared it with the Marsh fevers of other countries.

Many have found fault with the extent & minuteness of our study & the vulgar will always despise the minuteness of Naturalists, for a publication of 6 volumes in quarto on the subject of Insects. In Botany 10,000 plants are enumerated & described,

300 only of which are useful in Medicine.  
 A Physician may content himself with the  
 smaller number & pay no attention to the  
 others, but the knowledge of the whole may  
 be useful & we are ignorant where that Utility  
 may occur, <sup>they all may be useful</sup> and if I want to distinguish the  
 300 to communicate them to others, I must  
 study the whole, for Naturalists <sup>know etc</sup> that no single  
 plant can be characterized without a knowledge  
 of the rest; it is the same with the objects of our  
 study, for a disease can never be accurately  
 characterized and distinguished without attend-  
 ing to other diseases, and comparing the whole  
 of their character together.

It is necessary then that diseases should be ac-  
 curately distinguished, & the only means of effect-  
 ing this is to follow the Botanical method, to  
 dispose of diseases systematically into classes  
 &c, in order to render their characters more  
 accurate & complete. This was formerly a pro-  
 posal of the accurate & sagacious Sydenham  
 in the 13<sup>th</sup> page of his preface, After mentioning  
 in his preface this respects in the History of Dis-  
 cases

cases he proposes this scheme to remedy it, & in this he was seconded by Morton & Baglivi. A long time however elapsed before any thing on this subject was attempted, till Sauvages in 1731, with the approbation of Boerhaave, began it. Boerhaave however was sensible of the difficulty of the undertaking & agreeable to his judgement the first Edition of the work was imperfect. It was published in 1739 under the title of *Pathologica Methodica* where it was extremely defective, & even his 3<sup>d</sup> Edition is no more than a *Quodocimo*.

Physicians have declared the whole attempt to be trifling and useless; I shall not attempt a refutation of this, but beg of you to rest your determination on the utility you shall hereafter experience from it. Physicians of the latest standing are apt to condemn all new improvements in System from the apprehension of a discovery of their ignorance, and that the errors they have so long persisted in may by this means be pointed out.

The system of Nosology I have to offer will be new to such Physicians, as they are not



destitute of all method in Natural history, it is not to be wondered at that they condemn a Methodical distribution of diseases. I shall accede the Authority of a celebrated writer Origanus who (from his 83<sup>th</sup> to 84<sup>th</sup> Paragraph) has laid down reasons why diseases should be systematically divided, & in what that method consists.

Boerhaave has been forced back to Felix (Pater) who indeed arranged diseases, but his method is but indigent & not at all conducted by the rules that have since been prescribed by the Natural Historians.

Sauvages considerably enlarged his work, in an Edition of 5 Vols Octavo, & in his last Edition, of 1768, in quarto, he has extended to Species & added some useful Improvements. The first is the work that you are chiefly to trust to, but being inconvenient to consult on account of its bulk, I have abstracted the most important part of his work, & added to my Synopsis. I have carried Sauvages no farther than his 2<sup>d</sup> over, but to this I have added two Systems that have lately appeared, the one by the celebrated

Linnaeus

Linnæus who for accuracy in method is inferior to none in the present age; the other by Vogel of Göttingen, and by this you have at once seen every publication that has appeared on the subject.

The systems above mentioned have undoubtedly great merit, it is however incumbent on me to point out their defects. They are indeed erroneous & defective, but this will be more excusable when we consider that successive attempts alone can complete it. I propose to explain in what manner the Synopsis is made up, & shall afterwards mention the manner of using it, but first must proceed to say something in Method in general.

A digression of this nature may appear superfluous, but I presume there are many Gentlemen here who are unacquainted with the rules of System. Tho' the study of Botany may have preceded your attention to Physics, yet I am sensible that many apply themselves to that part of natural history without attending to Method abstractedly. I consider myself as in the situation of a Commander of a Convey to think it is incumbent

inconvenient on me to wait for the slowest Sail-  
 or in the fleet.

Method is a natural operation of the human  
 mind, which every one is more or less in pur-  
 suit of; Let us suppose a Man dropt from  
 the clouds endowed with sense & reflection  
 but entirely unacquainted with the produce of  
 our globe, in what manner would he be able  
 to distinguish between the diversity of the ob-  
 jects presented to his view? If he is dropt into  
 a park where he sees perhaps 100 animals  
 feeding on the herbage, the whole being Deer,  
 for instance, & perfectly similar to each other,  
 at first he will experience a variety, but on  
 reviewing the whole he will experience a great  
 uniformity & from the whole acquire but a  
 single Idea.

From the Deer let him pass into a flock of  
 Sheep, a difference in these from the former will  
 immediately be evident, but still from these  
 different observations he will have acquired  
 but two Ideas and two forms that run thro'  
 the whole.

Thus we form the first Abstraction & Generali-  
 zation

ization & from hence we acquire our first Idea of species which is divisible only into Individuals distinguished Numerically.

He next goes perhaps into an adjacent Forest and perceives a Herd of Stags; at first sight he imagines these Animals to be similar to the Deer, but perceiving a difference in the size, in the horns &c he observes that tho' a diversity takes place yet they have something very much in common that distinguishes them both from the sheep. If again he should find an African Boe which tho' in some respects differing, yet upon the whole resembles the former, which we shall suppose to be European, he will find that what these have in common will be sufficient to distinguish them from the others. Here then he will generalize & abstract, & arrange the ~~whole~~ into two different aspartments which we do name Genera.

This is the foundation of Method & the principal terms used are Genus & Species. We call that a Species that admits of no subdivision except to an Individual, which is usually impossible.



to the species in every mark & character. A genus admits of Affinities which may be subdivided without coming to Individuals.

The Logicians are still more minute, but a further division is superfluous & unnecessary. Genera are to Species as Species to Individuals, as Species unite Individuals so do Genera unite Species.

We shall next suppose a Dog to be the object of our stranger's attention; in this Animal he experiences a still greater diversity, he is destitute of Horns, & has 5 toes with claws, & hence is distinct from the Species & Genus he before observed. He finds that the Deer & Sheep have many things in common but different from his Dog, & consequently the former will admit of a higher Affinity. If he sees a Fox he will find from the resemblance between these two Animals that they may both be arranged under one head or united into a Genus entirely distinct from any of the former; he will also perceive the Agreement between a Cat & a Lion which are both different from a Dog, but from their greater

greater resemblance to the ~~latter~~ latter Animal than to sheep & they will all be arranged under a higher Asportment. Naturalists have accordingly given to these higher Asportments, the peculiar names of Order. Thus the Lion, Cat, & Dog Series.

In the Birds there is a material distinction from any thing our Stranger has observed. The first thing that will naturally strike him is the soaring of the Birds in the Air, while the Beasts creep upon the Earth; it will be obvious to him likewise that the latter have 4 feet & the former but two, & the possession of these things in common by the Quadrupeds, but different from the Birds. It will be a sufficient distinction for him to unite them into a Class in opposition to the Birds. And thus from the properties that Animals possess in common in opposition to vegetables, a still higher asportment will be made into what is by Naturalists termed a Kingdom.

These are the Natural Operations in which we proceed in distinction; we first proceed according to the Analytic method, i.e. from a collection

of particulars to establish general rules, but in the higher genera we follow a different method & proceed downwards, agreeable to the Synthetic from generals to particulars, thus from Quadrupeds to Ferae, & Pecora, from Pecora to Cervis & Ovis &c. & from these we proceed further to mark individuals.

Logicians speak of these Arrangements as Genera infima & Suprema. Naturalists have however found it necessary to affix particular names, thus a Genus being composed of Species is called Genus. When Genera are assorted, as in the union of Deer & Sheep, they are called Order. The assortment of Quadrupeds from Birds, Rept. Animals, as opposed to Vegetables, forms Kingdom.

The number of these Divisions between Kingdom & Species are by no means marked or ascertained, but are variable & arbitrary. When we define a Species which is the ultimate end of our labours, we define it by its Genus & specific difference.

Naturalists define a Deer *Cervus corvulus* &c. <sup>manus</sup> here *Cervus* is the Genus & all the words after added are what we call the specific difference which gives the characters by which the species is

is known; thus "*Cervus cornutus vamosus*" distinguishes it from the Camelopard &c; "*recurvatus*" from the Boabuck; "*compressus*" from the Chain Deer.

This is the manner in which we proceed, we give the definition of a Species by naming its Genus & adding the specific difference. We add the name of the Genus because it is easily remembered, & hence the name of Kingdom much easier; but in species the word "*Dama*" distinguishes it only from the other species of *Cervus*, & *Cervus* distinguishes it from the other order of *Pecora*; but it must be distinguished from all Quadrupeds by the order of *Pecora*, & distinguished by its character from every class, & also from Birds, Fish, &c; & as the one character is commonly employed yet the real marks of distinction are from its division into kingdom, class, order, genus, &c; thus a Deer is an Animal of the class of quadrupeds, of the order of *pecora*, of the genus, *Cervus*.

It is necessary to observe that this as well as every other part of what we call System is of the greatest utility, it is a natural operation of the



the human mind which comes are more accurate in marking than others, & is of the utmost importance in investigation.

Suppose one of the 10,000 plants be presented to me, & I want to discover it's name; if I am unacquainted with the method I must consult the description of the whole, which would be infinitely laborious, but if I have recourse to the methodical distribution the trouble will be shortened. Instead of consulting the whole, I have only to consider the 20 classes into which they are arranged, I then reduce it to it's order, & lastly to it's genus, by which my object is reduced to a few different species.

By the assistance of Method we can communicate our knowledge to the most distant posterity, & with respect to Insects, Plants, &c it's utility has been universally acknowledged. But in the other classes of Naturalists fewer species are to be met with; in Animals not above 200, & Buffon thinks the labours of Linnaeus frivolous in arranging what may be so easily remembered; but tho' it's importance with a view to investigation may not be so apparent, yet it is essential

to more full & accurate Observations.

The 8th has been referred to the order of Pecora, but it has been disputed whether the 8th is a ruminanting Animal or if it has the characteristic mark of the species of the *Caruncula luttoralis*; but if method had been in view these questions could never have subsisted, & its character would have been long ascertained.

Many admit the advantages of method to the Animal & Vegetable Kingdoms, but raise a subtle objection with respect to its further utility that it is only applicable to those beings that continue & propagate their kind, that it will not apply to *Fossils* & will less to *Minerals*. To this I will only allege that altho the distinction of species is not so well ascertained in *Fossils* as in Animals & Vegetables, yet a Methodical Arrangement of these has been attended with considerable advantages; respecting diseases I shall not so confidently express myself, yet an Arrangement is undoubtedly applicable & from Experience you will be convinced of its utility.

I shall now proceed to a particular consideration of Methodical Nosology.

In my part I have assisted you by giving Synonyms, & where no Synonyms are put down a new Class is established, to which Authors have been altogether inattentive - Thus, the 2<sup>d</sup> Class Neuroses, has no Synonyms, because it is new & unnoticed by writers. I might indeed have added a Synonym from Linnæus who comprehends a 15<sup>th</sup> Class under the Nervine, but I did not adopt this from an apprehension that it might be confounded with the modern Nervous fever.

Besides the Nosological Systems I have referred to the Synonyms given by the three great Systematics Hoffman, Stahl, & Boerhaave. With respect to the references made to Syntes, the numbers following his name refer to the numbers of his table. These numbers of his table are generally wrong, marked, but it is only by a single number proceeding from two different Editions of his work. These Synonyms I would recommend to you carefully to consult, as by these you will have an opportunity of collating the 4 Systems together; for investigating the errors I have presented you with an Index & references to the Systems from which

which they are taken, & the numbers in the Index are constantly the numbers of the genera of the respective Systems. Linnaeus & Vogel have numbered the Classes & orders in an irregular series, but Sauvages has arranged them under classes, the Genera of the latter are put down in the Index from the first to the last.

In all the words of a character every word has its precise meaning, if any are omitted the author has had his particular reason for it; one author has left out a character, because he did not think it necessary to constitute a Genus; another has added it from a contrary motive, & whenever you observe this difference of character you must think & find out the reasons for the change.

Sauvages, for instance, has divided his Class of Febres into 3 Orders, Continues, Premillentes, & Inter-millentes. Linnaeus has divided the same Class into Continues, Premillentes, & Exacerbantes, & what answers to the Premillentes in Sauvages, Linnaeus puts last of all. Vogel has divided his Class into two, Intermillentes & Continues, here the Premillentes are left out, & where Linnaeus & Sauvages have premised the Continues to the Intermillentes Vogel has followed



followed the contrary method. To solve this we must enquire, what he does with the *Permittentes*, he makes another Order of the *Continua*, & what have been called *Permittentes* he calls *Continua*. In our part under the first Section of *Solres* we have included among the Synonyms the whole of these different divisions, & also these differences of System lead to Pathological questions of the highest importance.

In attempting a *Methodica Taxology* the first thing to be considered is, what is to be comprehended, what is to be marked, under diseases, & what are to be taken in as Genera? 1<sup>st</sup> Principle, Any thing that can be considered as a disease sh<sup>d</sup> be comprehended in our System.

In the best of our Pathological Works, as of Morton, Sydenham, &c, good characters & distinctions of diseases are given, but their labours extend to but a few diseases, a System requires a collection & comparison of the whole in order truly to ascertain the characters of each; thus in the Animal Kingdom for instance, we establish an order of *Beasts* consisting of our domestic Ruminant Animals. If this is attempted by a tax

only acquainted with the produce of Europe, his order will consist of Sheep, Hags, Deer, &c. &c. and these have been characterized by their Horns but inaccurately, for others in Africa are destitute of horns yet still of the order of Pecora, the African Camel & Bush Deer are exactly conformable to the others in Structure & Nature, but have no horns. The analogy will apply to diseases, in the arrangement of which we shall ever be uncertain, till the diseases of every age & country are explained & considered.

Every thing therefore of the form of a disease should be comprehended. Authors however have gone to an extreme that is excessive. The whole XI class of Vogel contains too many minute deviations from the standard of perfect health & might very well be neglected. Of these his 163 genus, Seline, Myopias is, &c. are frivolous & trifling. Sauvages & Denon have fallen into the same error, & forgot the proper definition of Disease; every deviation from perfect health is not to be considered as a disease, but only such as are Lesions molests & permanentes, &c. some of the Martelle on many occasions may be con-

considered as such.

Again, Genera are unnecessarily multiplied by making a genus of a particular Symptom, & from inattention to this the Systems before us are rather to be considered as Symptomatology than Nosologia; for a disease should be formed of a concurrence of symptoms, or if it is defined by a single one it ought to be a primary & essential Symptom, thus many of the genera of Jaundices are only symptoms or parts of other disorders & therefore can never stand by themselves as diseases. I have endeavoured to put at the end of the Synonyms all the genera I think symptomatic, but this is a nice question & I should not be inclined to hazard such a general list, I have marked however the places where such a discussion is necessary, as in P. 257, under the order of Febres, I have considered Plethoræ as symptomatic & given a mark that it is so. Again under the order of Leanthemata I have considered the Purpura of Jaundices & Colicæ of denno as symptomatic, & in the 272 P. at the end of the order of Hemorrhagia I have considered Hematemesis, Hematuria, Cydorrhagia, the 3 first of Jaundices the last of regel, as hemorrhagic symptoms; & the same in several other places.

After

After the Dyspepsia & Hypochondriasis I had resolved to set down a long list of symptoms that other systems have converted into genera, but as these are subjects of dispute I thought fit to defer it.

Another fault in system is the making a species of one disease a genus. The number of higher genera between a class & species is variable & arbitrary, & the distinction of genus & species in pathology is extremely difficult & nice, I have often hazarded my opinion on this subject, as in P. 253. under Tertiana, I have brought together many disorders as species which are genera in other systems. I hope I shall prove the propriety of my alteration, none but the first are proper dynamis, the rest are species.

Further Authors in the enumeration of genera have considered as separate & distinct what are evidently parts of the same disease; thus Inflammation & Abscess are symptoms of the same disease in succession, the latter is always preceded by the former, & it must be obvious that these can never be separated but considered as a single disease.

In P. 258, after delivering the species of Inflammation, I have mentioned its consequent symptoms, Gangrene & Phacelus, apart; these are to be considered but as



as one disease, tho' I have marked them in different Letters. In 261 I have mentioned *Peripneumonia* & *Pleuritidis* *sequela* *aut.* *Thoracica* & *Empyema*, which Linnæus & Sauvages have formed into separate Genera: & in 271 I have made *Pthisis* a consequence of *Thromptoe*.

Genera have on many occasions been improperly multiplied, particularly with respect to internal diseases which are not to be distinguished by symptoms, as in Vogel's 60 Genus of *Mesenteridis*, *Col.* *Omentitis*, *Peritonitis*, *Myocarditis* *Pancreatica*, *Nephritis*, &c. &c. Tho' these are real diseases, yet they are unnecessarily introduced into a system, for they are not distinguished from each other, & hence are to be considered as the same, hence you will see the reason why I have given fewer Genera than the other Systematics. Sauvages has determined 315, Linnæus 325, & Vogel has unnecessarily augmented them to 560, Mine are only consist of 132, & the Genera of the others are comprised either as Synonyms, Species, Symptoms, or Sequela.

After determining what different *lib.*s are to be taken in, we shall next enquire what Characters diseases

diseases are to be made up of, and these characters are to be made abstracted from all Theory, merely the objects of simple sense rather than the smallest inferences of reason, & a Nosologia is to be considered as a collection of Medical facts. In many parts of Sauvages, Theory is introduced, as in his Class of Bilia &c; which are merely pieces of Theory determined from the internal state of the Body, & not from objects of sense. Linnaeus is likewise faulty in this respect, in *Plac. Gen.* 283. 284. he defines *Cherries Struma*, *Glandula indurata* & *infarcta*, these terms are to me quite unintelligible & his *Spiloch* & *Glandula* convey a nice distinction that can never be considered as an object of sense.

The great desideratum in Nosology is to select the characters that are always concomitant with the disease & inseparable from it. Certain diseases consist in a series, as others consist in a concurrence, of symptoms present in succession & these must have a progress before we can attempt to ascertain them. Thus Intermittents and the various remittents cannot be distinguished on the attack of the disease. We cannot be sufficiently acquainted with them till two Paroxysms are passed, & the

the periods of these accurately marked.

Linnaeus, under his first class *Exanthematici* has mentioned fevers & eruptions, but he has delivered characters to his fevers that are by no means applicable; thus *Febris synocha*, *junestissima*, &c. The marks of diseases that are to be taken from external symptoms ought also such as are, steady & constantly present, & it is of the utmost importance that such symptoms should be expressed clearly & without ambiguity. Vogel is in many places ambiguous, as in his term *Calor dolorificus*, & Linnaeus in his 238 Genus, the definition he gives of *confluentia* is by no means clear & distinct.

The Language of Pathology is far from being properly defined, & even a Linnaeus, notwithstanding his usual accuracy, on many occasions, employs very indefinite terms, as in his 8<sup>th</sup> Genus *Stomatina* & 10<sup>th</sup> *Macula* he employs the epithets *dilatabilia*, & *dilatabilis*, & throughout the whole of pathology the terms are by no means duly distinguished.

The *Philosophia Botanica* of Linnaeus, in which  
he

her has defined every term that is applied in Botany, is of the utmost utility, such a work in Physics is wanting, and every one engaged in the study must be sensible of the defect we labour under in this particular.

A circumstance of importance to be taken notice of is, that the natural affinities should be studied as much as possible, & on the contrary the artificial affinities of diseases should be as carefully avoided.

The advantages of these two methods must be obvious to every botanist; but, in a nosologia, necessarily alone, can excuse the employment of the latter. The system of Sauvages, though denominated, even in this respect extremely faulty; the class of Dolores in each is an artificial character, & by that mark different diseases are connected together that in themselves bear not the least resemblance. The term Dolor is too much in common to diseases to be employed as a character; it may comprehend the whole of the Phlogistici, & is improper on account of its ambiguity, for every feeling the human body



is susceptible of, may be comprized in this term.  
My 4<sup>th</sup> class of Locales is artificial, but I found  
it to be absolutely unavoidable.

In order to form a proper judgement of systems  
it is necessary you should understand the proper  
meaning of a character.

A character is not a description but a definition.  
A definition merely takes in the phenomena,  
that are absolutely necessary to distinguish a  
disease from any other, & neglects those Sym-  
ptoms that occur to it in common with others,  
hence in a definition we are to look for the  
essential & pathognomonic Symptoms that are  
peculiar to the object we are about to define.

Authors however have by no means strictly  
adhered to this in their characters of diseases.  
The 15<sup>th</sup> genus of Sauvages, Phlegmonia, is by no  
means characteristic, Tumor, sphaeroides  
&c is unnecessary, & is rather a description of  
Symptoms in common than a proper distinction  
of that particular genus. His 7<sup>th</sup> genus, Testula,  
Caricula ulcerosa &c is full of superfluous  
characters. Linnæus also in Syphilis, & large  
character of the febris hectica &c.

Systematics however, notwithstanding these instances, have rather trespassed in the contrary extreme, and given characters that are extremely deficient, as the 41<sup>st</sup> Genus of Sauvages (echinocela), which he defines "lyctis in seroto," & other numerous instances.

Another part of Method, besides accurate distinction, is proper denomination, for which in the science of Botany the critical balance of Linnaeus must be consulted, & for diseases I refer you to the Prolegomena of Sauvages. Great regard should undoubtedly be paid to a proper nomenclature, but caution should be used in changing names, that are already established, for new ones; nor can this ever be admitted without absolute necessity. Vogel has been even wanton in this respect, & thro' out the whole of his system the most rampant & difficult names are introduced. If a new genus is introduced, a new term must be applied, or if we affix a different idea to it a new name may be admissible. The only term I have introduced is for  
a

a class, Neuroses, which is a non descript in Authors, & on account of its being new my affixing a term to it will be more excusable.

I shall now enter on the 1<sup>st</sup> part of the Synopsis, which I propose to pursue as the plan of my course.

Authors have proceeded on the plan of dividing diseases into Universal & Local, the former of which comprehends the 3 first classes in your Synopsis, the latter the 4<sup>th</sup> class. By universal diseases are meant those generally affecting the system or the greatest part of it, and under this title diseases are comprehended that affect the primary & fundamental powers of the system; the local are confined to a single part, or particular Organ. We may call those universal diseases that affect the organs of the circulation generally, or any affection of the brain or diminution or increase of its energy in different parts of the system; hence fever, apoplexy &c may be considered as universal diseases; hemiplegia as affecting only one half of the body may be suspected to be local, but

but in consequence of the fundamental & primary function, the brain, being affected, it may be considered as universal. Inflammation, tho' it affects only one part of the body, is yet an affection of the general powers of the circulation. But these distinctions I shall not prosecute with anxiety as disputes may occur relative to the arrangement & difficulty of the application. I have to observe that there is hardly any method I know in any parts of science that admits of an easy application of the three classes. Linnaeus has given one, but it is by no means applicable, and Sauvages and Vogel, sensible of the difficulty, have entirely omitted it. With regard to the classes the characters are sufficiently obvious, & the application generally easy, and at any rate there is no necessity to be anxious about the distinction of the higher genera.

With regard to these three classes of Universal diseases, the symptoms of all diseases are so many instances of the functions lost, and hence Physiologists have long ago distributed



distributed these to the 3 heads of the vital,  
 animal, & natural classes, hence we might  
 expect that diseases given the complice &  
 compound should arrange themselves under  
 three heads, & it would be convenient if we  
 could arrange them in this manner. For this  
 reason I have here attempted it, but I cannot  
 see how we can completely comprehend dis-  
 eases under the 3 general heads. The diseases  
 affecting the vital functions, as respecting  
 the circulation, belong to the first, and I will  
 admit another set of vital functions, tho' not  
 commonly admitted in Pathology, it will  
 comprehend also the Animal functions. The  
 3<sup>d</sup> or natural I can't find exactly distinguish-  
 ed, for these depend on the functions of the  
 circulation, and consequently cannot be well  
 separated from that part of the vital functions,  
 and for this reason tho' I have treated of the  
 faults of the natural under the 3<sup>d</sup> class Cach-  
 exie, yet it generally treats of the faults with  
 respect to Nutrition & Excretion alone, the  
 other

other facts are referred to the other two classes as they come under their several heads.

No other of the methods could have afforded us so proper an order in considering diseases. I could not have begun with the *Febris* of Sauvages. He calls them *Elementa morborum*, but these will apply but to few, & is mostly comprehensive of Chirurgical cases, of which I shall take but few into consideration. I could have followed as little the method of Linnaeus, for I could not have begun with the *Exanthemata* without dealing previously to the doctrine of Fevers. My order will always prepare you for what is to follow. —

Class I.

1

# Paroxysm of an Intermittent 90.

## Class I. Pyrexia.

### Order 1. Febris.

This is a natural class; its character is simple & applies to every species arranged under it. I might have begun therefore with the character of the class, & then enumerated the several species; but I think we are hardly overwise in delivering the history even of a genus without keeping in view the several species that are under it, & much less the higher Orders. The higher genera are always in some measure an artificial arrangement, & hence are in danger of misleading us. The terms Febris & Pyrexia have in common been almost synonymous. I however shall make a distinction & shall begin with describing the Paroxysm of an Intermittent. I shall here give you a model of the plan I intend to prosecute in my descriptions of Diseases.

The Paroxysm of an Intermittent commonly

# Paroxysm of an Intermittent.

91.

comes on, with a sense of lassitude, which consists of a sluggishness & aversion to motion, joined with a sense of debility & uneasiness on attempting it. This lassitude is attended with a frequent almost involuntary stretching & yawning, the pandiculation of muscles, the skin becomes pale over the whole body, but more especially in the extreme parts; the nose, the tips of the fingers, toes, & the nails lose their colour. The bulk of the body sensibly diminishes, the face collapses; knivings, that were formerly light upon the fingers, now drop off; & the veins, that in health were prominent & extended on the surface, disappear. The skin becomes dry & very much contracted, but the papillae from whence the hairs issue are more evident & remarkable, & this particular appearance has commonly got the name of goose skin. To these symptoms a sense of cold quickly succeeds, & a sensibility to that of the atmosphere. It is first felt in the feet with a peculiar sensation, as if water was poured along the back, but it soon extends itself & conceals over the body. The sense



of cold increasing brings on a tremor, which especially affects the organs of Voluntary motion, but particularly those parts of the system that are not in so exact an Equilibrium, as in the Under Jaw, & with these are joined rigour & convulsive motions with violent succussions of the whole body. When this sensation of cold is universally extended on the surface, but more especially in the feet, the sensation of these latter parts is abundantly told to the feet of another person. But in general tho' the patient is highly sensible of cold, yet the body to others is often of the natural, & even greater, temperature. When this has for some time continued it becomes gradually intermixed with a sense of heat which extends itself to the surface & extremities parts of the body. These fits of heat become at length more considerable, & in proportion to the increase of these the sense of cold & tremor is diminished till at length the heat is extended universally on the body. On the approach of the heat the colour & fullness of the skin returns, it is most commonly first, asendant in the face with evident marks of redness & turgescence, with a degree of heat.

## Paroxysm of an Intermittent 93.

The heat is but for a time attended with dryness, for soon a moisture comes on, & a copious perspiration is excited universally. When this has sometime continued, the heat & dryness of the body abate, & with this the perspiration ceases & the system nearly returns to the ordinary state.

This is the series of Phenomena that most generally characterize the Paroxysm of an Intermittent. McSaurages has comprehended the more evident symptoms in his 21. & of his 2. Class. I think, what he has delivered may indeed be sufficient for a character, but more is necessary for a description. I have first given the symptoms perceived by the patient, & which are obvious to every bystander; another series of the phenomena respecting the functions must also be observed by the physician.

The Succession above mentioned has given occasion to Physicians to describe 3. Kinds of Stages, viz. the Cold, the Hot, & the Sweating Fit. In common it is only distinguished by the name of Hot & Cold Fit; the time that each fit occupies the patient is various, & the limits not easily fixed; but we shall in common make use of the words  
dis.

# Paroxysm of an Intermittent.

91.

distinction, & call that the proper Hot fit from the coming on of the heat till the breaking out of the sweat.

We shall now proceed to the other Phenomena.

I. The Pulse. At the coming on of the cold fit the pulse is smaller, weaker, & more frequent; this change of the pulse is previous to any of the appearances above mentioned. But, as the cold increases, the change of pulse becomes more evidently irregular, unequal, & frequently intermittent; but when the heat comes on it beats more regular, stronger, & fuller, but still somewhat contracted within its natural size. In the decline it becomes fuller, softer, & then the perspiration breaks out, & at the subsiding of the sweat, except being softer & weaker, it is pretty nearly in the natural state.

II. Respiration. In the cold fit there is a constriction of the Thorax & the respiration is small, difficult, & more frequent; but when the sweat comes on this constriction goes off, and the breathing becomes stronger, easier, & more free. As the sweat goes on it is like the Pulse, easier, & on the decline of this returns to its natural state. An anxiety relative to respiration comes on & quickly subsides.

\* Upon the supposition that the Patient before his being  
attacked with the Disease had been accustomed to chew  
Tobacco, take Snuff &c.



subsides; and anewly likewise from sickness, depending on the state of the stomach, and on: & during the whole of the paroxysm the patient has an aversion for animal food, & the usual appetites for tobacco &c. entirely cease. As the cold fit advances a pause arises, which often proceeds to actual vomiting, which is more or less bilious. The pause seems to have a share in bringing on the hot fit & is always most troublesome & short the decline of the cold fit. As the cold goes towards the hot fit the vomiting is considerably increased, but decreased as the hot fit advances, & entirely ceases on the breaking out of the sweat.

In the secretions the changes are remarkable. The mouth in the cold fit is dry, & clammy, & continues in that state till the appearance of the sweat. The state of the patient with respect to thirst exactly corresponds; it arises in the cold fit with the sense of dryness about the mouth & throat, & in the hot fit the symptoms are a burning sensation in the throat.

The Urine, in the cold fit, is also pale & watery, in the hot fit on the contrary it is dark, but without any appearance of sediment, but in the sweat or the ultimate stage a copious sediment is commonly deposited. At the end of the paroxysm a small quantity

# Paroxysm of an Intermittent

96.

occurs, but rarely during the time of the paroxysm. Tumors in various parts are found considerably to shrink, but generally regain their tumid appearance in the last stage. Ulcers desiccate & dry up in the cold fit, but at the sweating period discharge their ordinary matter. The head ach but seldom occurs in the cold, always in the hot fit & then is attended with a throbbing & violent pulsation of the temporal arteries. At the approach of the sweat this symptom also disappears. The patient is frequently sensible of pains in the arms of the back, & sometimes in the cold fit the intellectual powers are affected. In the first stage <sup>diminished, but in the second it is considerably</sup> sensibility is considerably increased with respect to the intellectual functions; during the paroxysm the faculties of attention & recollection are impaired, & from the confusion of thought that occurs a delirium is frequently <sup>exhibited</sup>. This symptom is sometimes perceived in the cold fit, but is rarely in the advance of the hot fit. The cold fit comes on with a drowsiness & stupor, and a Coma that in some cases may be denominated Apoplectic; & various spasms & convulsive affections arise that continue till the

## Paroxysm of an Intermittent 97.

The commencement of the 2<sup>d</sup> stage.

The particulars I have mentioned comprehend all the Phenomena that occur in the paroxysm of an Intermittent; they are (different) in different degrees of Intensity, in different persons, & in different cases. The periods of their continuance are from 5 to 20 hours; Vogel indeed has extended them but to 18; but from Experience I find reason to give them a greater latitude.

When the disease proves fatal, the death of the patient occurs in the cold fit, without the hot fit being fully formed; but, previous to the death, the hot fit generally comes on. This however has been an object of dispute among Physicians, but I believe it may be established as an invariable principle that no person in the paroxysm of a fever dies after the hot fit is fully formed.

The consideration of Fevers, as entering into the greatest part of diseases to which the human body is subject, is a disquisition of the highest importance. I begin, for reasons that shall be  
perceptible

# Paroxysm of an Intermittent

98.

hereafter mentioned, with a single paroxysm of an Intermittent. I described the Series of Phenomena in succession, & marked the several combinations, at the same time attentive to the proportions with respect to one another, as to their presence or absence in different cases. There is hardly an instance of a fever consisting but of a single paroxysm: it frequently occurs however that the heat abates considerably, and the pulse nearly returns to its natural state; the patient appearing pretty free from any symptoms of Fever, & this we call a Remission or Apyrexia. The space of time from the end of one paroxysm to the beginning of another we call a Remission, but the space of time from the beginning of one paroxysm to another, after an interval succeeding, we call an Intermission. We observe that the Remissions & Intermissions are nearly uniform & the Paroxysm commonly returns at stated periods, but different in different cases. The most usual interval that occurs is 38 hours, & this we call a Tertian period. The terms Quartan & Tertian are to be considered by the exact measure of their



# Paroxysm of an Intermittent 29.

their distances in recurring. We call it a Tertian when the beginning of one paroxysm follows that of another at the interval of 48 hours. This is the most common & universal period, but another, <sup>and this makes a Quartan Fever, another stage at 24 hours</sup> frequently occurs at the distance of 72 hours, which we denominate a Quotidian. There are some again whose intervals are shorter than the Quotidian, & others longer than the Quartan; these however have been disputed, & I think it unnecessary to determine as to their existence, because they are anomalous & <sup>perhaps</sup> only varieties of the Quotidian & Quartan periods. The paroxysms in different fevers are of different durations, they continue from 5 to 25 hours, & any number of paroxysms that admit of intermissions have each paroxysm finished in less than 24 hours, but some paroxysms of fevers come to no abatement in 24 hours, & at the end of that period are renewed & run their course as before. The hot & the sweating fit however, though they are not entirely removed, yet suffer an abatement of their intensity, and these are termed Remittent Fevers; by Linnaeus & acerbantes. In certain fevers

Fevers the remissions are more or less considerable, and the return of the paroxysm is called Accession; this is more distinctly marked by the accession of the cold fit at the beginning. When the remission is without sweat, the symptoms, milder, & the returning paroxysm without any appearance of the cold, & only distinguished by the exacerbation of the hot fit, the disease is denominated a *Continual fever*. The remission in some cases is very difficult to be distinguished, & from this physicians have conceived the existence of a fever subsisting for many days together without paroxysms; this is the *febris continua* of authors; but, without entering into the discussion of the point, I shall only observe that in all my experience I never had an opportunity of observing a fever in that form.

The *Quartan fever* has a long interval & a short paroxysm; the *Tertian* with a shorter interval has a longer paroxysm. The *quartan* has its paroxysm longer in duration than either of the former, & when it continues beyond a period of 24 hours it very frequently & gradually passes into a *Remittent*

# Intermittents

101.

Remittent fever. The tertian often changes to a quotidian, & the latter is frequently converted to a remittent of the most continent kind. These fevers preserve a great affinity with each other.

The history of Intermittents should be collected with the utmost care; Authors have been by far too concise in the enumeration of symptoms. The best descriptions that are extant are in the Aphorisms of Boerhaave, Hoffman, & in Sydenham's Diseases of Minoraca. Another Author must by no means be omitted, though his name is not affixed to his book. The person I allude to is Senac de Becard's *februm Intermittentium natura*.

I shall now proceed to investigate the causes of the phenomena, which will better enable us to understand & apply them. I shall begin with the proximate cause, previous to my entering upon the 2<sup>d</sup> Title, the consideration of Genera & Species is an accurate distinction of which the Proximate cause is of the highest importance.

The order I shall pursue in every disease is the following:

1. 2c

1. Definition.
2. Description or History.
3. Proximate Cause, to assist us in
4. Genera & Species.
5. Remote Causes.
6. Prognosis.
7. Methodus Medendi.

### The Proximate Cause of Fevers.

Now I must enter upon Theory; there is no part of our physical reasoning but what is complicated, but if these are carefully separated into distinct branches the complication that appears in the whole will not be perceived. For the security of our reasonings on this subject, I shall be more intent on the establishment of facts. I don't pretend to show the necessary connection between the cause & effect. The whole of my reasoning consists in the study of any two facts so connected in point of time that one follows the other, it is consequently the effect of the other, that precedes it. If we can but ascertain the

result



reality of our facts, the connection between effects & causes will be left difficult to explain.

The investigation of the proximate cause is a difficult problem that has hitherto baffled all the attempts of Physicians. I cannot expect a solution of it free from obscurity, but some acquisition will be made by a nearer approach to the truth, & by determining the method by which the problem can be solved. I shall at least bring this matter to a more simple question, & get rid of the false theories that have misled Physicians.

The first difficulty seems to arise from hence: The phenomena of fevers are numerous & various; & we cannot easily refer the whole to a common & single cause; yet from the phenomena being so constantly combined, one cause may be presumed for the whole; & as the series of symptoms are in regular succession it is presumed they depend on one another. I confess it is by no means improbable that the causes may be compounded & many of the Phenomena enumerated depend on causes accidentally concurring

concurring with the common cause of Fever.  
At present however we shall not embarrass  
ourselves with the supposition of a compound  
cause, but enquire concerning the common  
cause of the whole combination of Symptoms.  
I shall leave it hereafter to determine what  
are the Symptomata causa & what are the  
Symptomata accidentalia. If you are desirous  
of considering a System that is built on the  
notion of a compound cause I refer you to  
Quosnai.

I now proceed upon the supposition of one com-  
mon & simple cause of the whole Symptoms com-  
bined together in fever, & here I coincide with  
the opinion of pathologists that Symptoms  
are dependent on each other, & of which  
some are primary & fundamental. Many have  
asserted the increased action of the heart to  
be the principal & leading Symptom in fever,  
the hot fit was with these especially called  
Fever, and hence the causes of this have been  
considered as the causes of Fever. Fever is  
caused

sidered in this view must be occasioned by the application of direct stimuli to the heart & arteries. The distinction of direct & indirect stimuli <sup>become</sup> must be sufficiently obvious. The former alludes to whatever is immediately applied to the Heart & Arteries; the latter implies an affection by communication. — The cause of fever then has been imputed to direct stimuli, but with great impropriety, for tho' they are often applied & the action of the heart in consequence increased, yet they cannot be said to produce Fever or any disease. The newspapers say that by walking could increase his pulse to 120 in a minute, but it can subside without any further inconvenience; it is evident that the heart can be excited by wine & other direct stimuli without a disease being formed. Every increased action of the heart can never be denominated Fever; to constitute a disease the effects must be permanent, & consist in certain circumstances in concurrence or succession. As to the nature of these, many disputes have occurred; the universal & common opinion has

even is probable, that the increased action of the Heart & Arteries, following the cold fit, constitute a fever; no one can explain why the increased impetus of the blood should produce a cold fit, & therefore we must seek for other causes more adequate to the phenomena than direct stimuli.

We shall observe in what manner Boerhaave has proceeded, and opinions & reasonings will always be more intelligible by being contrasted with opposite opinions. Every one should be acquainted with the system of Boerhaave, this must be necessary, tho' the study of the doctrines of Galen may be dispensed with. Boerhaave then enters into the supposition of compound causes, that while one causes one the cold fit another acts upon the heart as a stimulus. Vid. Boerh. Aph. 576. 581. 582. If you attend to him here you will find him considerably embarrassed in point of system, & he is entirely at a loss which opinion to adopt. Van Swieten has taken up with the latter supposition, but has not totally explained how the one is produced & is a consequence of the other. The first question



question is, which of the two states precedes; but of this Van Swieten is entirely silent. It is probable however that Boerhaave supposed a direct stimulus to be the fundamental cause of fever; and that the irritation of the heart was the circumstance primarily produced, but as to any explication of the cause irritating the heart both have been silent. In what manner did they imagine the cause that they introduced, viz, a stagnation of the fluids in the extremities, was efficient in the production of the hot fit? They could not explain it, their cause was manifestly insufficient, & Boerhaave was sensible of the necessity of recurring to some other; in his 759 Aphorism, he imputes the cold fit to an inertia liquidi nervi; but here he is considerably embarrassed & altogether in a wrong train, for he is wrong in his order of precedence, & did not attend to this, that what is first in the ordinary times is first in ~~the~~ cause & effect, and as the cause of fever first operates in producing the cold fit, the increased action of the heart that follows must depend on the cold fit.

This

This I am disposed to assume as a fundamental proposition in the Investigation of Periodic causes, and that this is the cause of the consequent that fit is probable from the universality of the fact. The testimony of Hoffman in the first page of his first volume is strong in support of this, but the peculiar inconsistency of Boerhaave & Van Swieten on the subject is astonishing. Boerhaave in Aph. 756, is as explicit in favour of the opinion we maintain as can well be conceived. In this Aphorism, according to him, the cold fit is the *prima causa* & fundamental circumstance on which the other phenomena depend, on the removal of which the rest of the Symptoms go off. His commentator has been also more particular in the explanation of this doctrine; yet after all, the cause is limited to Intermittents; but it is evident that whatever is admitted as the cause of Intermittents may be admitted as the cause of Fevers universally; & hence the cause of the cold fit must be the cause of every other febrile Symptom. (The sentiments of Hoffman coincide in

in the same opinion; & now supported by authorities in the fundamental maxims of our System, we shall with confidence proceed in tracing its relative phenomena.

According to Bellini & Boerhaave the symptoms of the cold fit were occasioned by a viscosity in the blood, which rendered it impermeable to the minute extremities of the capillaries & hence it is gnated in the vessels. The imperfection of a doctrine built on so slight a foundation could not escape the discernment & penetration of Boerhaave, who sensible of its insufficiency imputed it to the *Inertia liquidum nervosi*. — Van Swieten proceeded still further & altogether resorted the viscosity of the Arterial fluid, & alleged that the phenomena depended on a change in the Nervous power. Astruc, in his treatise on the Nervous diseases, expresses himself in this manner, "that the sense of cold is not owing to the stagnation of the fluids in the small cutaneous vessels, but is occasioned by a spasmodic constriction of the vessels &c." Astruc. Astruc. I consider it therefore as a point established by Boerhaave

Boerhaave, Van Swieten, Hoffmann, & Whist, that the cold fit is an affection of the Nervous System, & this is connected with the most important doctrines in modern pathology, & is the source of the chief difference in the present from the ancient doctrines. About the time of Hippocrates there was some attention paid to the motions of the System (the impetus facientis) as the cause of diseases, but with little success; nor did Erasistratus & the methodic sect consider it to better purpose. The attention of Galen was confined to the quality of the fluids, & to an active power, which he calls Nature, that directs the motions of the System. This was the progress of opinions relative to the causes of diseases; it is agreeable to observe the progress of the human mind in improvement, as by this we can perceive the errors of the old, & the principles on which the new reasonings are established.

Till the middle of last century little was done by the ancients or moderns towards the organic System. Van Helmont indeed proposed a considerable alteration



alteration; by his Arguments he led to a consideration of the state of motion in the System & maintained that this had a greater share in the diseases of the body than the condition of the fluids which hitherto had been alone studied & attended to. His doctrine however was esteemed so fanciful that it was entirely rejected, nor had it better success though differently proposed by Boerhaave. About the middle of last century the pathology in all sorts was the same & consisted in a certain Imperies of the fluids. Fever was said to consist in a preternatural heat & it was not till the circulation was completed that this was rejected. Sydenham & Boerhaave found Fever to consist in an increased impetus of the blood & a quickened pulse. Boerhaave adopted the notion & thus made a step beyond the ancient doctrine; but the principle was not sufficiently extended, for, if fever was only an increased velocity of the blood, the means of cure would be merely to diminish the velocity, but this is insufficient, for it is frequently necessary to increase the impetus by Art, and

Physicians

Physicians at present are of opinion that the increased velocity of the blood is salutary & an effect of nature to remove the disease.

This evinces the necessity of tracing the Cause of Fever farther than the increased velocity of the blood. Bellini & Boerhaave, who have had the greatest share in forming our present system, have adopted the notion of Rent & acrimony as the cause of fever (U. B. Aph. 598.) & their method of cure is to dissolve the Rent, & correct ing & expelling the acrimony. Hoffman however has given us a different Idea of this subject & we must acknowledge him as the great Reformer of modern pathology. He first inculcated the general doctrine that health & disease consisted in the motions of the System (U. Ch. W. de Gonorrhoea, Methodum, Sect. 8.) where he lays down his general doctrine of what he calls the foundation of health, as it consists of the systole & diastole in the solids & progressive motion of the fluids. This is directly to our purpose, & properly understood, amounts to this - that the state of the body in health & disease depends fundamentally on

on the state of the Nervous System, on which the moving powers in every part of the body depend. This is at present so obvious that it is astonishing it could escape the attention of Physicians, for altho' Willis, Boerhaave, Marten, & Baglivi first made attempts to establish a Nervous Pathology, yet it was Hoffman who effected the most important change from the beginning of Physics to the present period. Agreeable to this doctrine Hoffman considers Fever as owing to a change of motion in our vascular solids, or to an affection of the fibres of these, which in effect is on the Nervous System - & supposed the Proximate Cause to consist in a Spasm of the extreme Arteries, entirely neglecting the state of the fluids. Consonant to this his method of cure consists in resolving the Spasm by remedies peculiarly adapted to act on the Nervous System. This doctrine is just in the general position relative to the Nervous system, & tho' it is long since it was established it has been little prosecuted by Physicians, hardly even acknowledged. He was unable indeed to extend it, & could only establish some general truths, without a sufficient enumeration of particulars. The apology of van Swieten, insi-

mular

similar circumstances, may be suitable to Hoffman & ourselves. (l. 578.) The doctrine of Hoffman was laid aside & his truths neglected because some difficulties attended the application of his system; at present however physicians are more sensible of its importance & have come pretty nearly into his opinion.

Boerhaave himself, from a conviction of the influence of the Nervous system, has added to the 2<sup>d</sup> Ed. of his Aphor. (755.) an affection of the Nervous system, besides the viscosity of the blood. Here indeed a hint is only suggested, but his commentator is much more explicit. He affirms that by the *viscositas fluidi arteriosi* must not be understood a change in the condition of the fluid itself, but whatever prevents its free passage thro' the Vessels. He demonstrates that Intermittents cannot depend on the circumstances of the circulatory or secretory systems, but on the *vasa liganda* *Encephalo*, which is plainly referring it to an affection of the Nervous system. (l. P. 580.) We have now brought the progress of opinions with respect to fever, so far as to find that the Boerhaavians coincide in our doctrine relative to the Nervous system: how this produces the



the cold fit & rather (Phenomena I am now to consider.

You may be surprised to see me so anxious to prove what apparently seems to require none; but my motive is merely to assist you in the solution of any objections that may possibly occur to you. I would rest nothing on mere authority; but when I am opposed by men of judgement & reflection it is incumbent upon me to pay attention to their arguments. Tho' I am conscious my opinion is well grounded & supported by the greatest authorities, yet candour must extort the confession that it is by no means universal, & an author of the highest reputation in physic has expressly opposed it. The person I allude to is Senac; who, as he is the first in honour, so I esteem him the first in point of erudition, in France. In his *Traité de la fièvre intermittente*, he observes that the cause of Intermittents has been referred to the nervous system, but affirms that the advocates for this doctrine are rather triflers than physicians intent on the cure of diseases. All this is alleged against the doctrine of van Swieten, & contains a strong declaration

age 1

against the opinion we allowed to be so well  
grounded & obvious. But his opinion in this  
respect will have little influence; it is easy to  
throw an imputation of mystery on the Nervous  
System, & to declaim ag<sup>t</sup> cause & sensible re-  
medies; but the confusion may have arisen from  
the obscurity of the subject, not when properly  
investigated it will more clearly elucidate dis-  
eases than any other doctrine hitherto adduced.  
When Mr. Lenax feels the pulse at the wrist & feels  
it to be weak, he concludes the contraction of the  
heart to be ~~weak~~, & this is a cause, more sen-  
sible obvious as much as from a weakness of a  
muscle, he concludes a weak influx of the Ner-  
vous power - for the action of muscles depend  
as much on the Nerves as the pulsation of Ar-  
teries on the heart, & they are equally objects of  
sense, with the other parts of the body. If diseases  
may arise from an affection of the Nervous power  
alone, there can be no hesitation in admitting  
it as a cause of diseases. It may with equal  
justice be admitted as the cause, to a change of  
weak Lenax ascribes the cause of Intermittents,  
a cause as much removed from our senses as  
the

the Nervous power; & upon the whole, if Senac had described the variety of diseases that arises from affections of the Nervous System, he would certainly have found less difficulty in admitting it in fever. But he himself, in his 5. th. 24. Paragraphs, falls into the very error he condemns, & considers as equally as Van Swieten & ourselves the cold fit of fever as an affection of the Nervous System, so strongly that I shall adduce some of his Arguments in illustration of our doctrine.

I conclude then that the cause of fever is to be sought for in the cause of the cold fit, & shall proceed to enquire into the proximate cause.

### Recapitulation.

In investigating the proximate cause of Fever I have made two steps,

1. That it is the cold fit of fever that properly constitutes the disease, & that this is the foundation of the hot fit & the other series of phenomena that follow.

2. I have maintained that it is an affection of the Nervous system, & this I have endeavoured to prove by the arguments adduced from Van Swieten. He observes that the phenomena of the cold fit

come on so suddenly that they cannot be imputed to any change in the sanguiferous system, but must be referred to an affection of the nerves or moving powers, which are so readily & easily changed. It is often brought on by circumstances that don't act on the fluids; as the passions of the mind, whose operation can only be referred to the nervous system - to these I add another consideration, that as the circumstances of the cold fit manifest a change in the motions of the body, these changes must be produced on the moving powers, and as every animal action must be performed by muscular fibres they must from the dependence of these on the nerves be referred to the nervous system; hence likewise the motion of the heart, & the blood which depends on the heart, must be influenced by the affections of this system.

I thought it necessary to establish the doctrine as fully as possible; if you look into Boerhaave's & van Swieten on continued fevers, you will find no mention made of the nervous system, but much upon Sordes, viscidities &c. In the subject of Intermitents you will experience a different res-



ry, but as in the first case he makes no use of his general principles, we may conclude them to have little foundation or utility.

Having thus established that the cold fit of fever is an affection of the Nervous System, we shall next enquire wherein it consists, & endeavour to trace it to its origin.

I agree with Boerhaave that it consists in an *Inertia liquidi nervosi* or a debility of the Nervous power; the latter is the expression I shall use in preference to the *liquidum* of Boerhaave. To explain what I mean I will suppose it to be understood that whilst the motions of the System depend on the action of moving fibres, & that these again depend on an energy of the brain which we denominate the Nervous power, The Sopor that begins, & the insensibility that is often accompanied, the cold fit, evidently shew that the Nervous power is considerably weakened. A debility also with respect to the heart & arterial System occurs, for the paleness of the extremities and the weakness of the pulse shew that the blood is not propelled with its usual impetus. Hence I conclude that debility forms the beginning of the cold fit of fever

fever, & lays the foundation of all the phenomena that afterwards follow.

In this part then our difficulty in theory begins; we are to show in what manner the state of debility is brought on in the heart & arteries, & how this produces an increased force of action. This is as difficultly that I shall not attempt completely to explain, but shall refer it to the general law of the system. The Animal Economy is so framed, that on the occurrence of any deviation from its ordinary state, this very deviation necessarily produces a deviation to that part, to restore it to its own balance, in the same manner as ballast to a ship tends to restore it to its erect position when the wind has occasioned an inclination to either of the sides. The Animal Economy, I suppose is so framed that whenever a deviation happens from its natural state it has a tendency to restore itself to its former condition. This is what constitutes the *vires naturales* medicaltrices, the *Ανοξαγεια* so named in the phrase of Physic. You may if not sufficiently acquainted with the nature of this power, consult Dr Haubius's pathology (from C. 38 to C. 50) where you will find the general fact sufficiently established; the nature of this law has been

difficultly

differently explained, but we are not here particularly concerned in the different ways of explaining it which would evidently lead us into a separate consideration of the subject. I shall here assume it as a fact that is everywhere admitted, & shall say that when any thing has been employed to weaken the Nervous power, if it has only gone a certain length it produces a reaction of the brain or an increase of its energy. And if it shall appear that Sedative & Refrigerant powers operate in this way & prove seemingly stimulant, as Opium & Cold which are manifestly Sedatives, prove commonly Stimulants, the analogy I think may be fairly transferred to the present state of fever. This fairly explains how the cold fit brings on the hot fit by referring it to a general Law of the system.

I will proceed a step further. The three natural Medicaltrices have been explained two ways. Stahl & his followers have resolved it into the powers of the soul acting for final purposes & not from any necessity of the system itself. But there are others who say that whatever concern the soul may have, yet its operations are constantly connected with a certain Mechanism of the corporeal parts acting upon each other. —

But

But without discussing the merits of the 2 opinions, the last is universally to be tried, that we may see how far it is a piece of Mechanism. Therefore I shall go farther than the Stahlians & enquire into the seeming mechanism that takes place in fever, as how the cold fit produces the hot fit. It is attended with particular circumstances which may throw some light on the subject.

The weakness of the heart & arteries that occurs in the beginning of the cold fit, accounts very well for the fulness, collapse & even constriction on the surface. For as the Animal solid is elastic & the degree of tension in the extreme vessels is owing to the blood propelled into them by the heart & arteries, if this be withdrawn the extreme vessels must be considerably constricted, & this in consequence of their simple Elasticity; but there are also many reasons to think that the extreme vessels are endued with Muscular contractibility which produces a more permanent kind of constriction in these vessels. for if these vessels were only constricted by their simple Elasticity they would soon be restored to their former dimensions upon the renewal of the force of the heart; but it evidently appears in the hot fit that



that the action of the heart is restored, but yet the constriction is not removed or taken off as appears by the dryness of the skin, the mouth, & drying up of ulcers, & collapse of tumors, which show that the constriction remains a long time after the action of the heart is restored. Hence we conclude that it depends on the contraction of the muscular fibres, which is the Spasmus Periphi. This contraction may be at any time brought on by the application of cold to the surface, & this is the cause of the cold fit of fever, whose action can in no way be accounted for but by producing debility; now as this constriction produces a reaction & increased impetus in the part which is evident by the glow induced after handling snow &c, so cold may be applied to the whole so as to produce a constriction universally over the system - & by a general law of the Economy a reaction or effort is produced to overcome the constriction, which produces the hot fit with the several circumstances attending it.

Thus then we have in some measure attempted to show the phenomena of the cold fit.

I consider the Remote Cause of Fever to be a poison, which weakens the Nervous power in

consequence of wch the action of the heart & larger vessels is weakened; the blood is hence not propelled to the extreme vessels with it's usual impetus, & these are contracted by their muscular coats & natural elasticity combined. A sense of cold accompanies the constriction, but this is partly owing to the Nervous power being withdrawn. This altogether produces a reaction of the Nervous power of the heart & arteries, & the constriction remains till it is taken off by the increased action of the heart when a sweat breaks out.

Some difficulties may attend this doctrine of Mechanical Constriction but these are not so considerable as those attending any other notion that has been offered, & it does not rest it's merit on the mere idea of the connection between the several parts, but it is really established in fact. The debility is followed by a Spasmodic affection of the smaller vessels which brings on an increased action of the heart & arteries, and this continues till the energy of the Brain is restored & the constriction removed from the vessels.

(Fever then consists of three parts

1. Debility

1. Debility.
2. Spasm.
3. Increased action of the vessels.

As these are successive in the order of time, so to each other they are mutually causes & effects, & from hence we may understand the import of several parts of practice that have been pursued.

I have now given you my theory of Fever, I deliver it as a doctrine to me extremely probable, but would advise you to receive it with caution; and that you may so receive it, I shall mention the difficulties attending it.

**Objections to the Tricimate Cause of Fever just now advanced.**

First, as I said that fever consisted of 3 states, the two last are the most frequently observed & therefore most evidently take place. Dr Hoffman goes no farther than this. In Inflammation & Hemorrhage, two orders of the Crisis, we observe the Spasm and increased action, but any previous state of debility does not appear, and hence it may be said as Spasm arises here without Debility, may it not also occur in fever without debility? and therefore only as Dr Hoffman supposes Spasm and Efflu- But-

But this affects us little, for when we come to Inflammation and Hemorrhage we shall find that these admit of an explanation on our System; they have a previous debility or what is analogous to it, but even supposing it, it could not amount to more than this that Pyrexia depends on Spasm, & there is a difference between Inflammation and proper fever.

A debility is always present when there is a languor, lassitude &c. of the voluntary Muscles, together with Anorexia &c. which are symptoms of the same, being present long before any thing indicating Spasm, and though nothing shows it so evidently in hemorrhage and Inflammation, yet it is no argument of it's not taking place in fever.

Some of the Remote Causes are of a debilitating Sedative nature, which we cannot perceive to act any other wise as causes of Spasm than as this follows from causes of Debility already explained. —

Supposing the causes of Fever to be direct causes of Spasm, we cannot suppose the spasmodic efforts should produce first debility and immediately afterwards increased action; a double operation of this kind is scarcely credible.

What,



What gives us the greatest difficulties is the operation of cold, which may be supposed to produce fever, now the operation of fire is contrary to debility, but when the operation of cold is properly considered we are quickly relieved from this embarrassment.

1. When cold alone act. as a body or as to nature. By vesic it is a fire. Inflammatory kind. I am of opinion that it is never productive of permanent effects without previous symptoms of Debility or some local affection.

2. If it be true that cold does not produce a simple fever but where debility & other causes have preceded it will establish our theory. That it will produce fever where debility has preceded is very obvious; its effects are hardly considerable, except when it acts on a body considerably weakened. Every one must be sensible that he has often been exposed to cold, even to considerable incursions of it without any bad effects; hardly ever is fever produced by this means without great evacuations. Debility from Mind, Senery, Convalescence &c. concur. Hence the causes of fever act in producing Debility because when carried into the body they render it liable to the action of cold. contagion and Miasmata have frequently been taken into the body and no bad effects

effects have followed till cold concurred. Hence we have this explained by the general doctrine laid down that Contagion introduced produces Debility, but is not sufficient to produce Spasm unless cold be applied, and then that degree of cold, which otherwise would have produced no evident or considerable effects, produces spasm.

I shall urge the Objection still farther, it may be alleged that extreme cold applied by itself does often produce all these symptoms, which we have explained as the effects of Debility, and hence that it may produce all the effects observed in fever.

To this I answer. Cold in many cases acts as a debilitating power, or a powerful Sedative, and destroys life altogether without producing Spasm which acts as a Stimulant, and hence Cold in a certain degree, short of that above mentioned may produce a Debility, and after that on the continuance of its application produce Spasm, but it never produces Spasm without first producing Debility, but even if it could it would be unable to produce fever unless Debility had preceded.

If we allow Spasm to produce Debility it does not, alter our doctrine, it still consists of 3 parts, for if

if Spasm one while proves a Stimulant, at another a debilitant, yet different Indications will arise from its diversity of Action.

Whatever difficulties may arise from the consideration of Cold, and suppose we should allow in certain cases that Debility and Spasm arise from Cold, yet in certain other cases we find Contagion of itself sufficient to produce febrile paroxysms without the operation of Cold at all. Hence I conclude that our doctrine is probable that there exists in fever at different times with different proportions of Duration

Debility,

Spasm, and

Increased Action.

It might be desirable to advance a step farther and explain the foundation of this connection; here I acknowledge that much difficulty may occur, and I shall not think it safer to enter far into the subject at present. We can easily understand how debility by weakening the action of the heart and Arteries gives occasion by drawing the blood from the extremities to induce a Spasmodic constriction, but

how this spasmodic Constriction produces a Stimulus to the system and produces a reaction I can only account for by referring you to the wise Nature Medicatrix, wherein a sense of this Constriction by an inexplicable law of the System produces the reaction that is necessary, it is enough that we know the law is universal to refer to it. I might however say that merely a sense of debility is a condition of the same kind & brings on the necessary reaction, but how to account for this on Mechanical principles is difficult; that it is produced partly by debility & partly by Spasm, but that debility produces the reaction by the Intervention of Spasm.

Many other questions might here occur on this subject, as how the increased action of the heart & Arteries takes off the Debility and Spasm which were its causes; this I defer to an after consideration, I conclude however that these three states take place in fever; that Debility especially takes place I conclude.

1. From the consideration of the means by which every fever is produced.

2. From a consideration of the remote causes which



which are all of them of a debilitating and sedative nature.

3. Because a state of Debility especially takes place with respect to the Animal functions, and continues during the course of the fever, hence we might enquire how increased action takes off Debility as well as Spasm. We find an explanation from certain symptoms.

1. With respect to rigor, horror & tremor, which make so considerable a part of the cold fit of fever so that some Physicians have considered them as constituting almost the whole of the cold fit. At other times they are the consequence of external cold, and they have been considered as the effect of cold that always accompanies the first stage of fever and produces no other effects than a sense of cold it does; but other causes beside external cold may bring on this tremor, as fear, but the fear may also bring on cold, yet it sometimes brings on tremor in a greater degree than it does cold. We must here consider wherein it most especially consists. Physicians have supposed Tremor is of two kinds, Convulsive and Paralytic; but with what propriety they have considered the former

as Convulsive. I shall not now enquire, but I am certain of 20 cases that occur 19 at least are Paralytic, or symptoms of other diseases of a debilitating nature; hence Mr Sauvages has improperly put it among Convulsive disorders, Gaubius with more judgement has made it a symptom of Atonia. I consider it in the latter view, viz. as an effect of Debility. Every one's observation will suggest proof enough of this, thus tremor under a state of Debility only occurs when we attempt to move our members but when supported this tremor does not appear. This tremor I contend arises from a constant alternation of the weakness and the efforts of the Will. To apply this there are many instances of a motion of the energy of the Brain that is without any conscious concurrence, of the operation of Volition, thus the weight of the under Jaw is supported by the muscles, if these are over-acted by Siccure or Sleep they fall down: so in the case of debility there is a constant inclination to fall and as constant an inclination, though without our knowledge, to support it; hence tremor is a constant effort of the system when endeavouring to a reaction, whilst it is under a state

State of Debility, and thus tremor in the cold fit of fever is a mark of the reaction which is to follow being already begun. It is not merely a symptom of Debility alone, but this is mixed with a constant endeavour to support the tone of the Muscles. This is reasoning theoretically, but I shall bring out my conclusion to some matter of fact.

I say it is a mark of the Reaction being begun, for as this tremor is more and more strong & much sooner will the Spasm be overcome & the fever admit of a solution. The tremor is hence more considerable in an Intermittent, than in a continued fever where the Spasm remains a longer time & the paroxysm is of a longer duration. Mr Senac has marked some facts much to my purpose, in page 20 de feb. second: nature: where he observes that fevers which have great rigor & tremor in the beginning, will nearly all of them admit of a solution into the Intermittent kind.

The horror, tremor, & rigor is always most considerable in those Intermittents that have the shortest paroxysms, hence it is most considerable in quartans, less in tertians, and still less in Duodecians.

Critical Accessions that give a solution to the disease are commonly distinguished by some extraordinary horror and tremor, these are then not to be considered merely as symptoms of Debility, but of the reaction of the System, and from the condition of these we are led to judge of the condition of the disease that is to follow.

There some difficulties <sup>will</sup> arise from Boerhaave's Aphorism 782. I think by *ex febris prior* he means the cold fit will be severer; and with respect to the *pulsus citatus, debilis, parvus* &c. they are in greater degree they will show the fever to be the worse. But this is not the case with respect to horror, tremor, rigor &c. as in the quartan, when the paroxysm is shorter, so much severer is the horror, tremor, rigor &c. In 623 he says *freque febrile* &c. but this last Aphorism must be understood with regard to the *burn of cold* or the actual cold produced and observed by a bystander and unaccompanied with tremor, rigor &c. Whether the plague has any rigor, tremor & horror I won't say, as practitioners have not marked it with exactness. Petrosalvus de febre pestilentiali, says that whilst there was a slight horror present there was so great an abstraction



abstraction of Heat that the limbs seemed, as it were, frozen with cold. Many other proofs might be drawn of an actual coldness that often takes place in the feet, and this symptom usher in fevers of the worst kinds; but the horror and tremor are to be considered as a mark of the reaction being begun, and according as it becomes more considerable the paroxysm will be milder. Van Swieten only refers to Sydenham's account of the plague, who says that some of the worst cases come on without any cold at all, and this he supposes to be the case from the general Tenor of Nature, but does not observe what a connection it had with the event of the disease. Though all horror, tremor & being on dangerous fevers, yet when the reaction is begun the debilitating cause may still prevail and determine the after state or consequences of the disease. The facts of Cheneau on the subject of the plague are strong in confirmation of our general doctrine.

We now shall consider another set of Symptoms relative to the state of the Stomach.

An affection of this viscus is so constant an attendant

attendant on fever, and so intimately connected with the general affection of the System in which fever consists, that its discussion is of the utmost importance.

Pathologists have had recourse to a particular matter lodged in the Stomach, which, by the irritation it produced, occasioned fever. Hence the theory of the Bile being a cause of fever, is generally admitted both in ancient and modern times: but the whole of their doctrine is rendered doubtful from the Stomach being often disordered by affections of the remote parts of the System. Vomiting is often produced by Sympathy; and in a delirium Animæ, in consequence of Generation, where, there is no direct action on the Stomach, it frequently occurs. As soon as the blood ceases to flow the delirium comes on and the vomiting that frequently follows must be produced by the general debility, at that time prevalent in the System, and hence whatever occasions a certain degree of weakness in the body may be a cause of vomiting, and this general Symptom in the cold fit may be considered as the cause. An obstinate vomiting is a Symptom of fever, which with other marks demonstrates a great degree of debility; but besides this general weakness it is probable that some circumstances in other parts have a share of the effect of vomiting not only de-

(a)  $u$ , when the hot fit is fully formed.

depends on a general debility of the System, but also on the particular state of the surface of the body, the connection of each with the Stomach is too well known to render any further explanation requisite.

There are some circumstances occurring in fever, even in the cold fit, occasionally vomiting to take place, whilst in the hot fit it entirely ceases. This is the state of the fact, and it serves to demonstrate the dependence of vomiting on the constriction or spasm on the surface. If this is removed, and the circulation is fully established on the surface the vomiting immediately ceases. This is beautifully illustrated by a fact of Dr Sydenham, he says that he was disappointed in the use of Sudorifics, by the vomiting that accompanied the fevers, which immediately evacuated them from the stomach. After mentioning the Diaphoretic Medicines he proposes to employ, he observes that without restoring the motion of the blood to the surface he could not get the Medicines to remain on the stomach, owing to the constriction on the surface depending on the general state of debility which this was an effort to remove. This particular symptom of vomiting therefore is a confirmation of our general doctrine of debility & spasm, the particular operations of which in producing vomiting we shall not attempt to explain, we rest contented with the fact, which is all that is necessary for our purpose. — at



At length, after numerous Authorities adduced & objections refuted, our doctrine of fever is established. We alledge that it consists in three different states of the System, Debility, Spasm, & an increased Impetus of the heart & arteries. These different conditions are the same as to cause & effect, as they occur in the order of succession in the disease.

Tremor, which is a mean between Rigor & Horror, I have alledged to be an Index of the reaction of the Sensorium having already commenced. This will admit of a particular application, & we shall point out a view of this subject different from what Systematics have delivered. Boerhaave has proceeded on an erroneous plan, for from observation it is evident that the tremor is always in proportion to the consequent reaction of the System; if the former is considerable the reaction will be greater, & be attended with a more complete resolution of the Spasm. This is agreeable to the observation that the Tremor is always most considerable in those Intermittents that have the shortest paroxysms & admit of the readiest solution, whereas in those that have short tremors the reaction is incomplete.

plete, & are liable, according to Senac, in diuturnitatem abire, & be converted into continual fevers.

The fact seems pretty well established, that vomiting is connected with the Spasm on the surface of the body. The causes of vomiting do not act immediately by producing the evacuation, but occasion a sickness & nausea. We find the nausea to be a symptom of the debility of the Stomach, and this is agreeable to a general law of the System which occasions the exertion in vomiting, by which the cause of debility is removed. That such reaction is produced is evident from the practice of exhibiting Ipecics in the cold fit, the effect of which is to promote the accession of the hot fit & more speedily to finish the disorder. The exertion of vomiting produces a flush or glow of heat over the body, & it not only interrupts but entirely terminates the cold fit. It is a fact that Ipecics tho' supposed to be Stimulants are in their direct operation Sedatives. Narcotics & other substances that produce this effect are Sedatives & induce a state of debility but their effects are counteracted by the general laws of the System which produce a reaction or effort

# Fever

140

effort to throw off the noxious matter. They are therefore, (to express myself more concisely) in their direct operation, Sedatives; but are indirectly, in consequence of the exertions of the System, Stimulants; & this is further confirmed from the most powerful Sedatives & Narcotics proving perfectly innocent. It is probable that the sickness that precedes vomiting is an effect of the debility & Spasm, & hence the Stomach must be considered as a fundamental affection. The Nausea & Vomiting is an effort of Nature which has the effect of restoring the energy of the brain, & of resolving the Spasm in the extreme vessels, in consequence of which the influx of the blood is restored to them. Here however I think it necessary to elucidate an ambiguity that may occur; Vomiting, tho' considered as producing a reaction & thus obviating the cause of debility, is however not always a favourable symptom; for when it is obstinate & proceeds to a considerable degree it is an expression of the power of its cause, viz. Debility, & is a sign of its being general in the system. The Nausea therefore that amounts to a smaller reaction & depends on the same

same cause, are to be considered as unfavourable prognostics.

The Bile has been generally considered as a cause of fever, & in vomiting, great quantities of bilious matter are evacuated; this renders the opinion of Physicians somewhat plausible that the Bile is the primary affection & has a considerable share in the production of fever. But it is now a general observation that warm seasons & climates produce a change in the condition of the Bile, that a Cholera Morbus or a copious afflux of Bile to the intestines is the peculiar effect of such climates & seasons. & Sydenham observes that the proper Cholera is confined to the month of August when the heat is more prevalent. Hagerhorn has observed in Minorca (a climate much warmer than ours) that the Cholera is a disease of the Summer or beginning of Autumn; its appearance will however be sooner if the season is hotter, but it most commonly occurs from the middle of July to the middle of September. This is the state of the fact, but the Theory of it is uncertain, as it is dubious whether the



# Fever

112

the Bile is secreted in a larger proportion, or whether the stimulus of its acrimony occasions a more copious effusion; It is probable that they both have a share. The cholera morbus frequently happens with fever either preceding it or consequent upon it; but the changes that occur in the Bile are by no means necessary to the production of fever. The circumstances are easily explained independant of supposing the bile to be a cause. In Autumnal fevers a copious efflux of it may readily occur, but this we presume to be rather a consequence than a cause of fever, for fever is attended with a considerable constriction of the surface & this accompanied with a debility of the heart impedes the transmission of the blood thro' the smaller vessels, & consequently an accumulation will take place in the larger trunks & particularly in the Venous System;—from the constriction of the vessels on the surface a determination must necessarily be made to the internal parts, to the viscera, & especially those of the Hypochondriacal System, where the venous blood is in greater quantity & of more difficult transmission.—  
Hence

Hence may be explained the frequent congestions of the Vena portarum in those that have died of Intermittents, & Cloghorn from a dissection of such a patient found the spleen to be swelled to a prodigious size—about 14 or 15 pounds— & of so soft & loose a texture that it had more the appearance of coagulated blood than of an organized part. This congestion in consequence of the cold fit will account for the more copious secretion of bile, and its afflux into the intestines, & no change is produced in the condition of the Bile previous to the fever. According to the observations of Cloghorn in the Sporadic or Vernal Intermittents there is no appearance of an over proportion of Bile, & hence we conclude it to be an effect & not a cause of Fever.

The efflux of the Bile may be accounted for in another way. Vomiting may be considered as a cause of it, for the action of this agitates & emulges the ducts, & the bile by the inversion of the Peristaltic motion is thrown into the intestine, & from thence out of the body; the effects then  
may

may be produced by vomiting & the reason that the evacuation is more remarkable in fever is evidently from the action of a cause that promotes its secretion; this is the state of Spasm, which from the principles above mentioned will occasion a greater determination to the Liver. In the Autumn likewise when the Bile is more copiously produced the effects will be more apparent, & the evacuation more considerable.

The opinion of the Ancients relative to the bile I pay little attention to; but Senac, who from his great Judgement has acquired a deserved reputation, must by no means be passed over. This vigorous author after rejecting the opinion of VanSwieten endeavours to discover the most probable cause, & affirms it to be the Bile: his fallacy however is obvious, & from himself I derive an insuperable objection to the doctrine he supports. (Méd. Senac. Chap. 2. de recandita &c. &c.) He acknowledges the fact that the superfluous bile which occurs in the Jaundice may be diffused over the system, without producing fever; he furnishes us also with another argument to the same purpose, which is so obvious

in favour of our doctrine that it is astonishing he did not apply it.

Upon the whole then Bile is inadequate to the production of fever, even in those that we usually denominate Bilious, & at present we are convinced that Intermittents are owing to something received from without, to the alteration of particular effluvia; & whatever may be the condition of the bile, if there is no inhibition of effluvia, fevers are never produced. — (vid. Lind's Treatise.)

(S) Senac is of opinion that Fever may be produced by the mixture of febrile Miasmata with the bile, & his doctrine is probably not destitute of foundation; for I do not alledge that the contagious miasmata act only on the Nervous System, they act likewise as a ferment on our fluids, & have an affinity with those more or less as they happen to be in different conditions, & it is probable the Miasmata may unite with the Bile especially in the Autumn, & be the cause of affections in the primæ viæ. This union of the Effluvia with the Bile is most probably the cause of Dysentery, (α)



a disease, that most commonly occurs in the autumnal season; but this is not always the case, for Dysenteries frequently occur without the smallest symptoms of Fever. In warm climates, where Dysenteries are, Epidemic fevers are rare, & the patient affected with the Dysentery is secure from the other disorders of the season.

Cleghorn quotes a passage from Celsus Aurelianus & differs from him in his assertion that children are more liable to Epidemic Fevers than Adults. He, on the contrary asserts that bilious diseases appear first among children, & from those being less liable to fevers, it is obvious there is a difference between fever & dysentery, & between fever & pure bilious affections. I have been more particularly explicit on this subject from & regard to the opinions of Senac. & Pringle, for both these are disposed to the same opinion, but, as only some hints are suggested, I shall omit any further discussion.

We now proceed to another set of symptoms that express the state of the Sensorium.

Every problem relating to this subject is of the most

most intricate Investigation; we shall prosecute however our usual plan to find out the matter of fact & what state of the System this condition peculiarly implies. As the greatest part of fevers are attended with delirium we presume this to be connected with the general affection of the System on which fever depends. In the hot fit when the head ach comes on, with a pulse full & strong, & delirium is arising, & this from the circumstances attending it, we immediately impute to the increased impetus of the Blood in the vessels of the Brain. If we consider that every Inflammation of the brain is attended with delirium we cannot possibly hesitate in admitting this as a cause, for if one species of delirium is owing to increased impetus, another may <sup>be</sup> reasonably supposed to be owing to the same.

Pathologists have been of this opinion & generally supposed delirium to depend on the state of the circulation in the Brain (vid. M. Aph. 701) after defining delirium he proceeds to the cause, but he has expressed himself in so general a way as to comprehend every thing on the subject

-ject. The plurima causa which Boerhaave had in view was evidently the state of the circulation, & increased impetus, for in the very next paragraph where he comes to his method of cure, his remedies (as Chrysmata diluentia pediluvium &c) are evidently such as diminish the increased impetus of the blood & it's determination towards the brain. Van Swieten is sensible of the necessity of going farther, and says a delirium may be symptomatic to the stomach, & the functions of the brain be affected independent of the circulation, but he had ~~no~~ no conception of a delirium being cured by the application of powerful stimulants. A gentleman who was subject to this disorder drank daily the quantity of 8 bottles of wine, & it constantly removed the delirium. We say then that the motion of the Nervous power can on many occasions be changed without any alteration in the state of the blood, & we must seek for the cause of the change in the intellectual functions independent of the state of the circulation - for in great exhaustions the

the impetus may be diminished, & in violent exercise it may be increased without any change in the intellectual faculties, & therefore I seek for the cause of the change in the Nervous System alone; this belongs to the Mind to the immaterial substance with which the body is yet so intimately connected with it as to follow the corporeal state. I suppose it to be sufficiently demonstrated (demonstrated) in physiology that the energy of the brain is continually fluctuating, & is communicated to different parts with different degrees of force. In this difference the states of Sleep & Waking particularly consist, & this different state of the brain is expressed by the terms Excitement & Collapse which are terms signifying a matter of fact & destitute of Theory.

From the phenomena of Sleep & Waking we know that those 2 states may be in different degrees with respect to the whole, & with regard to different parts of the System. We can easily understand how this inequality



of excitement in the brain in different parts can produce delirium. I shall illustrate it by an obvious instance. A person in falling asleep the transition from watching to sleep from excitement to collapse comes on by degrees successively in different parts. whilst a great part of the system is under a state of collapse some particular senses may have their excitement increased, & if external impressions are made upon our organs during this partial state of collapse, this inequality of excitement may produce a delirium, which will continue till the excitement or collapse is rendered more complete with respect to the whole; thus to a person roused from his sleep by unusual stimuli as pinching, loud noises &c the sudden excitement being not yet extended to the whole produces delirium. (hence the common expressions, that I am out of my senses, not awake &c.) This is the illustration of a fact, & it evidently shows that delirium always depends on some inequality in the states of excitement or collapse, & we presume this is all

all that is necessary in the explanation of  
Febriles delirium.

Debility we observed is the foundation of diseases,  
& this consists in the diminution of the excitement  
of the energy of the brain which often produces  
such a collapse as to bring on deep sleep or  
coma, & this state is unequal in different parts  
of the system. The collapse is much greater  
in the Animal than in the vital functions, & in  
different degrees of excess in the vital as well  
as the Animal. The inequality then of the  
excitement of the brain lays the foundation of  
delirium which may depend either on the excess  
of excitement or the excess of collapse — an in-  
creased impetus of the blood is a cause of a  
greater excitement, & on this cause the excite-  
ment in some measure depends. — When the im-  
petus is increased while the collapse subsists  
with regard to the Animal functions the system  
is irritated & a delirium will come on; this is the  
delirium that occurs in the hot fit. — The other  
species of delirium comes on when the system is  
under an excess of collapse, in such a state the  
impetus of the blood being increased may produce  
delirium

delirium, and this is that species that occurs in the cold fit.

Here then are two seemingly opposite causes of Delirium in fevers, the one depending on increased Impetus or Excitement, the other on Collapse, which is in its nature sedative. If this is considered with the general doctrine of Sleep & watching & with the intellectual functions, you will perceive its connection with our doctrine of Fever, which is strongly confirmed by this explanation of Delirium.

This is a sufficient explanation of the transitory cases of Delirium in the cold or hot fit of Intermittents which occur without topical affection of the brain & hence cannot be supposed Inflammatory. When the delirium however depends solely on a morbid affection, the brain may be topically affected & a true phrenitis may take place; other topical affections may also arise which may prove causes of excitement or collapse, & increase the general state of fever; other symptoms, & particularly sleep & watching that occur in fevers, may be explained upon the same principles.

Boerhaave accounts for the states of Coma & Per-wigilium from the state of the organization of the brain.

Brain; they may sometimes probably depend on topical affections of that organ, but if our pathology be rightly understood Coma will be found to depend on the Nervous System which alone the cause of fevers, & is productive of that state of Collapse in which Coma consists. The several transitory Comas in Intermittents are a strong proof of this, & is upon the whole consistent with our general Theory of the cause of fevers operating by diminishing the excitement & energy of the Brain.

I have now finished my general Theory, which I hope will recommend itself to you from its simplicity. It is totally divested of hypothesis, nor does it depend on an Acrimony or Lensor or on any subtle changes in our fluids, of which the facts are not properly ascertained. No subtle hypothesis of the Nervous fluid has been adopted; intent thro' out the whole, upon the investigation of facts I leave you to determine the Theory - whether you suppose it to be Spirit, Electricity, Magnetism, Gravitation, or the Universal Matter of which these are probably a modification; To me it is indifferent, my maxim is to investigate its laws



causes & to learn what truly happens rather than how it happens.

If you suppose Fever chiefly to consist in the frequency of pulse, my first step is to deny the effect of such increased action but when it begins with a Spasm on the surface. Hoffman entirely coincides with this opinion, (vid. P. 301. Vol. 1.) & most Writers agree in the same. Even Boerhaave alleges that Fever arises from an internal cause first beginning with horror, but how this Spasm produces the increased action is difficult to explain. I rest satisfied with the fact that it evidently precedes it. As to the cause of the Spasm I assert that it is not a direct Stimulus, but is founded on a state of debility induced by the several remote causes of fever. This I allege merely as a fact, drawn from the phenomena of the Animal System, the motions of the System are evidently weakened, & the energy of the Brain is weakened, & it is equally just to conclude a debility in the Brain as a weakness in the action of the heart from the smallness of the pulse. I have attempted a rationale of disease

ease, & reduced the causes of Fever to

1. Debility.
2. Spasm.
3. Increased Action of the heart.

The last is the effect of the other two, either of one or both, as they occur in different cases, or as, induced by different causes.

Spasm & Debility we consider as the foundations of disease, & what we call the proximate causes. In confirmation of our general System new symptoms have been considered & were applicable on the same principles, & still further tended to corroborate our opinion, the more so as the theories of other Systematics were insufficient & incomplete.

According to our general Theory the doctrine of Fever consists in Debility Spasm & Reaction, as they occur in the order I have mentioned them so they produce each other in succession. It is doubtful however how those states proceed in every Pyrexia, but the fact is demonstrable that they constantly occur, and are all that are necessary to constitute a Pyrexia. There can be no

I have given to the class is founded on this supposition, "post horrorem pulsus frequens, viribus artuum immixtilis," & in this paragraph you may observe how the different parts in some measure allude to the whole order of the Pyrexia.

If I had added two other words, 'calor auctus,' which might with propriety have been inserted, I should have agreed with both ancient & modern, as their character of fever is great heat & frequent pulse, but these are alone insufficient to constitute fever. The 571 Aph. of Boerh. leads evidently to a mistaken notion of fever, & our opinion is that only increased pulse, following horror & spasm constitute a Pyrexia. This class we have subdivided into 5 different orders, and here we have taken pains to show that when debility produces spasm the disease is strictly called fever.

But for a full distinction of the Pyrexia we have defined it to be without local or topical affection; this character secures it from being confounded with the 4 orders which are also distinguished

(a). I have added Hemorrhagia & Profluvia under the class  
Pyrexia, which are not inserted in the other Nosologies. Hoffmann  
indeed is the only author that has taken a proper view  
of active febrile hemorrhages, & from their connection &  
dependance on fever they should by no means be separated  
from febrile diseases.



ed from each other by the difference of the topical affection. The Phlegmasia are distinguished by Inflammation, the other two are distinguished from each other by the different nature of the fluid evacuated, the one consisting of an unusual quantity of blood the other of an increased excretion not naturally of blood.

The 1<sup>st</sup> cause then of Fever is a State of Debility.

2<sup>d</sup> Cause, productive of Spasm, we denominate Congestion, which is any accumulation of fluids in an unusual quantity; these cases of Congestion are applicable to the 2 orders Phlegmasia & Hemorrhagia. As to the Exanthemata I suppose a state of Mixture, & is so far to be called fever as it is preceded by a state of debility which produces Spasm & is always followed by a topical affection. I have some doubts as to the explanation of the 5<sup>th</sup> Order, Profluvia<sup>(a)</sup>; it agrees with the Exanthemata in having fever preceded by a state of debility; in other cases however I am uncertain whether the peculiarities of the Profluvia are owing to a direct Stimulus applied with the causes of Fever to particular parts of the body.

(a). The change of the term *increased action*, for *reaction*, will appear necessary from the consideration of its being a consequence of the Spasm, & its being an effort to remove the Constriction, agreeable to a general Law of the System. —

I shall now consider the Orders as they occur.

## I. Febrile.

In these the states of debility, spasm & reaction take place, & tho' we may consider them in the series of Cause & Effect, or as occurring separately, yet we shall find these states to be constantly present thro' out the whole of the process from its commencement to its termination. If the increased action or heat continues, without abatement, attended with a drying of the skin & thin & limpid urine, it is evident that the vessels are under constriction notwithstanding the impetus of the heart. Whilst Spasm remains, the cause of it, Debility remains, & the energy of the brain is not yet restored especially to the external vessels. The continuance of the last state of debility is evident from the weakness in the functions of those of the internal senses. Symptomatic affections of the Stomach come on, the appetite is lost, & the patient has a *Nausea & Eructus*, which symptoms may be considered as marks of debility in the neighbouring functions. The continuance of the fever is proportioned to the permanence of the causes

causes, and as these states may be in different degrees of excess, so the disease is always conformable in continuance, violence, &c.

Fever is obviously of two kinds, Intermittent & Continued, & this distinction has been observed by physicians in all ages. The Intermittents are such as consist of a repeated number of Paroxysms, between each an Apyrexia intervenes. The Continued I suppose to consist of regular paroxysms but without the intervention of a perfect Apyrexia or a remission of the paroxysm. This idea implies that in these fevers the hot fit is constantly more or less present, & they are only distinguished by the different degrees of exacerbation in that hot fit. When the pulse is at one time unusually increased it is called a Paroxysm; but if on the 2<sup>d</sup> day, comparatively with the former, it is diminished, we call it an Exacerbation. As these alternate remissions are more or less observable so other distinctions arise; when they are without the intervention of a perfect Apyrexia, but yet the alternate state of remission is more considerable, Physicians have called them Remittent.

Another



Another genus, under the Remittent, has been formed, that I presume is exceptionable & superfluous. In some Fevers the remissions are hardly observable, & the distinction between the paroxysm & exacerbation is difficult. Physicians however have refused the least symptom of Remission & affirm that there are fevers of the Continant kind of several days duration, during the whole of which it consists of a single paroxysm.

Writers have distinguished between the two terms Continant & Continued, meaning by the latter the continued remittentes where the exacerbation comes on once in 24 hours; but in the continant they consider the state of reaction as continued & uninterrupted, succeeding for several days together.

In every Intermittent that has a complete Anagoge the paroxysm is finished in less than 24 hours, & in every continued or remittent we observe an exacerbation to take place about the same period. We are not so conversant in the nature of fever or in the operation of its causes as to say how the paroxysm is so determined to return at a particular time, but we presume it depends on

on a general law of our system, that modifies every cause & determines it to a diurnal revolution, & fever is so modified from whatever cause it arises. Every diurnal revolution of our earth, with respect to the sun, by giving regular alternations of heat & cold, light & darkness, produces considerable changes in our system, & gives it a diurnal habit; the mere alternation of light & darkness has an influence on the regular routine of human affairs. This regularity of routine in a system that is easily subjected to habit, must have an effect in producing a diurnal habit. This however is reasoning from causes; but from certain effects we presume, from the vicissitudes of sleep & watching &c. that the body is subjected to such a habit. The pulse is subjected to vicissitudes that occur periodically. In the morning when we awake from sleep the pulse is low, but the least muscular action & even an erect posture increases it; notwithstanding however we continue in that posture, yet the pulse decreases till noon; after dinner in consequence of the digestion of Aliment it is considerably en-

creased

increased but subsides towards 7 or 8 in the evening & again gradually increases till the hour of rest. In sleep there is a considerable remission, but again a gradual increase till 2 o'clock in the morning, from which it gradually decreases till it falls into the state mentioned above. This habit in our system I presume is what modifies fever from whatever cause it arises, & we must impute the cause of fever rather to a general law of the system than to particular causes.

But however probable, I may have rendered my conclusion, yet as the question is about a matter of fact doubts may arise, especially as my opinion is contrary to the testimony of Physicians in all ages. To obviate this however you need only consider that the generality of Physicians are rather guided by the authority of others than by their own observations, & the opinion is so contrary to the operation of fevers in general that it should be most precisely ascertained to be admitted as an exception. Add to this that nature is so uniform in her operations that where any

any strong contradiction occurs to the general tenor of things they may generally be imputed to false & superficial observation. De Haen in his *Divisiones Februum* has made 4 Classes. 1. The *Continua non putrida*. 2. *Continua non remittentes* &c; but altho he has constituted a 2<sup>d</sup> class of fevers without remissions, yet he acknowledges the difficulty of absolutely ascertaining them, & has added a particular Scholium, in which he partly coincides with our opinion.

I now proceed to enquire into the causes of the principal forms of fevers, the most remarkable of which occur in the Intermittents & to these I shall chiefly confine myself. The paroxysm of the Quartan we observed was shorter than that of the Tertian, & this <sup>last</sup> still shorter than the Quotidian, & from the principles explained above it will be obvious that the shorter the paroxysm is the longer will be the interval or remission.

This principle, being laid down that the recurrence of the paroxysm depends on its duration is a considerable step in our doctrine of fevers. it establishes the general difference of Intermittents & Continued fevers: Every fever whose paroxysm



is protracted to 18 hours must have a recurrence in 24 which is exactly conformable to fact & no instance occurs to the contrary. It must be evident that if from any cause the fit of a Quotidian is protracted to 24 hours there can be no Intermission, because a new accession must come on at that time; & hence the fever becomes remittent which is a confirmation of our position that the recurrence of the paroxysm is connected with it's duration. The Quartan & Tertian by a protraction of the fits are changed into Quotidians & these into Remittents which depend on the paroxysm being determined to a longer or shorter duration. As to the cause of the longer or shorter duration of the paroxysm, it is of difficult investigation; the duration of the paroxysm has a relation to the cold fit, particularly to the degree of horror, tremor, that arises, & these have not only more considerable tremors than the continued, but differ in degree among themselves, the Quartan greater than the Tertian, the Tertian than the Quotidian.

The

The horror, tremor, rigor, I have endeavoured to shew are symptoms of the reaction of the system, & a remarkable state of these implies a more considerable reaction, & a more easy & complete solution of the spasm, & hence gives a paroxysm of a shorter duration. That the duration of the paroxysm is in proportion to the reaction excited may be difficult to prove, for if we judge of the reaction by the symptoms of the hot fit we shall find the frequency of the pulse, heat, &c. to be more considerable in a paroxysm of a long than one of a short duration. We often find them to be greater in the fit of a Quotidian than in those of a Tertian or Quartan.

Some other circumstance may be presumed to oppose the solution of the paroxysm, but it can be referred to no other cause than the constriction produced in the extreme vessels, in consequence of the spasm to which the continuance of fevers & the duration of the paroxysm are entirely owing; but here the difficulty arises, for if we consider the spasm to be the chief irritation to

to the sensorium, by which its reaction is produced, then the irritation should be in proportion to the spasm, or in other words the effect should be adequate to the cause. But the irritation of the spasm & the constriction produced are not always in the same proportion, we shall find it however to apply if we suppose the constriction to be moderate, & yet the irritation considerable, from which a stronger reaction is excited, & consequently a shorter paroxysm produced, & on the contrary when the constriction is considerable the irritation & its consequent reaction will be less, & the paroxysm be of longer continuance.

This doctrine of Irritation & Constriction, however, I must acknowledge to be hypothetical, but such reasonings are indispensable if they contribute to the solution of Phenomena & are afterwards converted into fact. Here then I must anticipate our Theory of Inflammation.

Pyrexia depends on a congestion of the Arterial System; this is irritation partially applied, which communicated to the whole produces Pyrexia or some specific effects in the Arteries themselves. This irritation applied to the Arterial System & considered as partial to these, increases the tone & contractile power

power of their muscular fibres, in consequence of this the vessels not only contract more strongly in consequence of dilatation but resist the dilatation of the heart. The blood is more closely braced to the capacity of the Arteries is diminished by the increase of the cold, & in this condition of the Arterial System the Diathesis Phlogistica consists. That such is the state of the vessels appears from the remedy we apply, for Trepanation, relaxes the Arterial System & cures the Phlogistic Diathesis.

Whenever the Spasm is productive of the Inflammatory Diathesis, it will occasion a more perfect contraction in the extreme vessels, & the fever will be of more difficult solution; this will differ in excess in proportion to the power of the causes producing it.

We shall endeavour to establish our doctrine by the following considerations.

I. The Pyrexia attending Phlegmasie are always of the continent kind, & tho' by the force of the Diurnal habit they have Remissions, yet they have no Intermissions.

II. We find that Stimuli applied to the arterial system is the common way of converting Intermittent to continued fevers.

( III. Fevers



III. Fevers are more commonly the produce of Cold climates & Cold seasons, in which Inflammatory diseases more frequently occur.

IV. Intermitents are more generally prevalent, in warm climates, which furnish the causes of debility with less irritation.

V. Venesection is not so necessary in Intermitents, but essential in Continued fevers.

The Spasm producing constriction being great (the irritation inconsiderable, we suppose to be the cause) of the long Paroxysms of Fevers, & to a similar cause we may refer those Fevers of the continued kind.

From the whole then we presume that Spasm producing considerable constriction, with respect to irritation is the cause of longer paroxysms & therefore of continued fevers, & that most part of these are joined with more or less of Phlogistic Diathesis.

I have been at pains to prove that the causes of Debility operate in every proper Fever, & operate differently in different cases, in some cases so considerable as to extinguish life without any appearance of

of Fever, & from the appearances in the dead body we observe evident symptoms of Contagion; this we must presume to arise from such causes as occasion debility.

In other cases the Debility is not so great as originally to prevent a reaction, & then some degree of horror comes on; still however the reaction is insufficient; & the patient dies before the advance of the hot fit, & hence the reason of Death frequently occurring in the cold fit. Other cases occur where some degree of the hot fit comes on, but the debility is so great that the energy of the Sensorium is not strong enough to produce a reaction equal to the removal of the paroxysm, & the patient dies in an intermediate state between the cold & the hot fit.

The irritation may be in different degrees & as the affection is distant from the common origin the energy of the brain may be communicated to the heart, but may not perhaps extend to the extreme Arteries. As the energy of the brain is not extended to the extreme vessels & the heart is weakened from the vigour of the Sensorium being not so strongly applied, the reaction will be hardly sufficient

sufficient to overcome the constriction, & hence long paroxysms will be formed, & thus from Debility taking off the determination from the brain we account for continued fevers.

In this manner then have we solved the problem relative to continued fevers. We first found them to depend on the long duration of the paroxysms, & the causes of this we have referred to an excess of Spasm or an excess of Debility. In Intermittents the debilitating cause is not so great as to take off the irritation of the Brain, nor is the Spasm so considerable as not to be solved by the Reaction.

We shall here more particularly distinguish two cases of Continued Fevers.

I. Where Phlogistic Diathesis concurs with the cause of Fever, i.e. Debility, we call it an Inflammatory Fever.

II. Where the Debility is so great as to take off the determination from the Brain & prevent its proper reaction, we call it a Typhoid Fever.

The Synocha or Inflammatory Fever is a disease of cold climates & cold seasons. It attends persons of a robust & active temperament & such as are liable

# Inflammatory Fever

171

to Hemorrhages. It comes on without much languor or other symptoms of debility preceding - it seldom arises from contagion, but often from the application of external cold. The cold fit has little horror & no tremor, it continues but for a little time & is not attended with vomiting. In the hot fit a turgescence of the face is evident, & symptoms of an approaching sweat. The heat is very great & equally diffused over the body; the pulse is full, strong & hard, a violent throbbing of the temples comes on, accompanied with headache & delirium.

The patient complains of pains in different parts & particularly in the small of the back; his breathing is short but not laborious, & the anxiety not very (not very) great; his appetite is not very great nor yet entirely lost, & a considerable thirst ensues from the heat, the belly is bound, & the urine is high coloured without any sediment. The disease generally terminates in about 7 days, but the Remissions are obscure & inconsiderable, & the solution is always by hemorrhage or sweat, after which the urine deposits a sediment.

These are the phenomena of the Synocha that are most frequently & commonly combined; for



# Inflammatory Fever 172

a further description of wch you may consult Dr. Hoffman who has more accurately enumerated the symptoms than any other Author. We suppose the Fever to consist in the excitement of a considerable reaction, while its effects are resisted by a permanent Spasm. An Inflammatory Diathesis or increased tone of the Arterial System prevails, as appears from the success of Venesection, & the Blood <sup>is drawn</sup> that has an Inflammatory Crust.

If we now take a view of our doctrine we shall find the continuation of Paroxysms to be owing to Spasm.

1. To a permanent & obstinate Spasm, &
2. To a Spasm accompanied with debility, in consequence of wch the reaction was inconsiderable & unequal to its removal.

The first case depends on the Diathesis Phlogistica, wch consists in an increased tone & contractility of the Arteries, & hence occasions the more obstinate Spasm. The 2<sup>d</sup> is the case of debility, & tho' the force of the Spasm is not so considerable, yet from the reaction not being duly excited a longer paroxysm prevails, & hence where symptoms of debility occur continued fevers prevail. This is agreeable to the common division of Fevers into Inflammatory & Nervous. I proceed to

a description of the latter.

## The Mervous Fever, or Typhus.

This is more frequently a disease of warm climates & seasons, & commonly observed to arise from contagion; it especially affects weak persons who are exposed to causes of debility & to the operation of cold. It comes on slowly & gradually & the patient is affected with a loss of Appetite, languor, lassitude &c for some days before the fever is formed.

A sensibility to the coldness of the Air comes on towards the evening, with a slight horror; this is succeeded in the night by a slight heat & confused sleep. In the morning the patient is relieved, but the same phenomena return & then the hot fit is more considerable. After 2 or more days the hot fit constantly continues, attended with a considerable degree of heat, but a moderate frequency of pulse, neither full nor strong. The Vigour of the Animal functions is greatly diminished & a considerable despondency of mind takes place; the appetite for food is lost & nausea & frequent vomiting arises; with all this the patient has little sleep & Delirium or Sympomania comes

comes on. The heat is unequally diffused, being less in the extremities than in other parts, & the face is seldom flushed. The belly is irregular, sometimes is bound & at other times a diarrhoea comes on, & the urine is pale without any sediment. The Hemissions that at first were observable become gradually more obscure & the disease is lengthened out to 2 or 3 weeks & ends without any evacuation or what is called a crisis. The functions of the brain are evidently disturbed by the Coma, Tremor, &c. that arise, & the whole of the phenomena are to be explained by a prevailing debility of the primary function.

If Fevers were uniform & steady in their appearance we should have less difficulty in distinguishing them, but the phenomena are very much diversified, & it has been frequently observed that the symptoms of the Nervous & Inflammatory fevers are very much intermixed. It is proper to remark that the causes of fever do not preserve the same tenor & condition that they first began with, for repeated paroxysms or the increased power of contagion introduced, changes the appearances & keeps up the fever. In some cases when the symptoms of the hot fit are more violent in succeeding Paroxysms the power of reaction

-tion increases & will at last overcome that paroxysm that at first it was unable to effect, & here the spasm is constantly diminishing; in this way we account for the spontaneous dissolution of fevers. In other cases however the repetition of the paroxysm is attended with an opposite effect, for it diminishes the power of reaction, & hence being unable to effect a solution of the spasm the disease becomes fatal. It is from the change of circumstances in continued fevers that these events are to be explained & we should separate those symptoms that are to be imputed to debility & those that proceed from spasm, & those that are <sup>to be</sup> ascribed to reaction or the relaxation of spasm, & refer them all to their separate causes. Some cases we shall attempt to explain.

When a fever first puts on an Inflammatory appearance, but during its progress assumes a Nervous form, we may impute it to its occurrence in cold countries where the debility is not great, & the application of cold occurs as an exciting cause; this combined with a vigorous constitution gives the Phlogistic Diathesis, & the disease comes



# Nervous Fever

comes on as a Synocha; but the repetition of the paroxysm induces a debility with diminishing the energy of the sensorium as well as the increased tone of the Arteries, the fever subsists in the Nervous form.

Let us observe what distinction Intermittents admit of, on the same principle. The difficulty is that Tertians & Quartans are often changed to Quotidian, & these again to Remittents of the most continued form. Of this there are two cases.

1. Where fever begins as a Continued & changes to one that has more evident remissions.
2. When the Fever begins as an Intermittent, and changes to a Continued.

We have an example of the first in the Camp fever of Dr Pringle; and of the latter in what are commonly called the Malignant Tertians, the same that Dr Lind has described. This disease is to be imputed to a powerful cause of debility, not ~~to~~ <sup>being</sup> combined with a constitution disposed to a strong Spasm.

The Miasmata or Contagion which commonly induce fever must be supposed to act on the Nervous System, in consequence of which they may give the  
several

several varieties of fever; besides however the action of these on the Nervous System they are often of the nature of ferments, & act on our fluids, & by this means contagions are modified and produce different effects in different persons. It is obvious that this may occur with little alteration in the state of our fluids, sometimes however the fluids tend to putrefaction, & the proper putrid fever is produced.

The *Febris Putrida* was commonly called so by the Ancients, but without any precise or definite idea being affixed to it - nor are the moderns, correct in this particular, for they have distinguished the putrida & nonputrida without determining the distinction by proper marks. At present we confine the appellation of putrid to those fevers in which the putrid disposition of our fluids is evident. The marks of such a putrescency I shall shortly take notice of.

### *Febris Putrida.*

If in such a state Blood is drawn from the arm, it does not readily congeal, & the serum is of a redish colour. If the Blood issues spontaneously from different

# Putrid Fever

different outlets, from the Gums, & if it appears in the feces without Dysenteric affection, or in the Urine without Symptoms of Nephritic, or by the lungs & pores of the skin without any pulmonary affection, then we may safely conclude a putrescence of the blood rather than an increased impetus. The blood is poured into the rete mucosum & erithemas, & Petechiae appear on the skin. Hemorrhages of the Nose also frequently occur, and if these are without the appearance of crisis they may be supposed marks of a putrid dissolution. These are the most certain Diagnostics. Other symptoms however appear, as fetid breath, an obstinate Nausea, & a cadaverous smell arising from the whole body. The effluvia of putrid Animal matter is one of the most powerful Sedatives, & a most deleterious poison, hence such sudden death ensues in consequence of a Sphacelus, & we presume that the putrid vapour will have an effect of the same kind & occasion that debility that so evidently increases in the course of such Fevers. A great debility certainly remains & I conclude that the Miasmata causing Fever are of a Sedative nature, but whether the increased debility is owing to

to a repetition of paroxysms or to contagion becoming more abundant & its further progress more powerful, or, chemically speaking, more exalted. Putrescency may be owing to a putrid ferment introduced, which affecting the nervous system may dispose the fluids to greater putrescency.

The several genera of Fevers, particularly the Typhus & Synocha are more or less connected with putridity & may be divided into Genera & Species. but the combination of fever with the other orders of the Pyrexia must also be considered. Fever is frequently joined with Phlegmasia, & a question may arise which of these two is the Idiopathic disease, this may be determined in the following manner

1. From the seasons of the year. The phlegmasia are more prevalent in the Spring, Fevers on the contrary in Autumn; this however is ambiguous.
2. From the first symptom, as they begin with the Fever or Phlegmasia, they are to be considered of the nature of the disease they commence with.
3. When the symptoms of both are simultaneous



we can only refer to the prevailing Epidemic.

11. Where the confusion is more evident, it is a presumption that Fever is the Idiopathic disease. This is the case in the 1<sup>st</sup> species of Scurvy the Synochus, & the Febris Pleuritica as described by Sydenham in 1673. first he describes a Fever, then an Inflammation came on, this again passed off & the primary fever continued; the subsistence however of the fever, when the pleuritic symptoms appeared, shew this to have been the original disease.

Fever is often combined with Exanthemata, many of which depend on a peculiar & specific contagion; to this head the Small pox & measles belong. other Exanthemata are produced independent of specific contagion, but in consequence of certain states & conditions of Fever as the Ecthyma, tho' in what cases each takes place is uncertain. The distinction is nice, & has occasioned a controversy between two Physicians at Vienna.

De Haen maintains that miliary Eruptions are symptomatic & merely the effect of the warm regimen in Fevers. Stark, on the other hand, says that

that they are Idiosyncratic & give a character to a particular Fever.

But if there is any truth in the observation of its having arisen in one part & gradually extended to the rest of Europe, & if it had been traced as contagious thro' several countries we must allow it to be owing to a specific contagion. It is however much oftener symptomatic as appears from several Arguments.

1. It is always in this country sporadic, & I never saw it properly Epidemic.
2. It attacks people in particular circumstances: Child bearing women are most liable to it.
3. It is always preceded by Sweat in severe, & in some persons it is always the consequence of Sweat.
4. There is no determined period of the Incubation, as the specific contagions have, & therefore we conclude it depends on a certain state of Fever & a particular state of the skin conjoined.

There is no species from the combination of Fever with Haemorrhagy, such are generally critical, except from a Dyscrasia of the blood in putrid fevers. There is an important combination of Fever with profluvia as in the Catarrh

& Dysentery, & it is doubtful which of these is the Idiopathic disease, but we may determine this in the manner we did with respect to the Phlogmasia, & I shall hereafter make it appear that the fever attending Prothruia is the Idiopathic disease.

I shall now proceed to give a general view of the System I have delivered.

### Recapitulation.

I have proposed to teach the Practice of Physic on a new plan, not so much from the peculiar doctrines I may have offered, as from my attempts to reduce it to general principles. These principles are nothing but matters of fact, & every thing merely hypothetical I have avoided; my reasonings are facts in themselves, or conclusions drawn from these, or facts generalised; & for a review of the whole, I shall briefly recapitulate the several parts.

I began with describing the leading Phenomena of Fevers, & proved that Fevers were distinguished by the different duration of their Paroxysms. I described the phenomena in their train of succession & in their variety of combination; but it being impossible to comprehend the whole

(of Morton, Sydenham, Boerhaave, Hoffman, Van Swieten,  
Senac, Pringle, Clegorn, Lind, &c.



# Recapitulation

183

Donumerate the varieties that may occur, I referred you to authors. In our further progress you may observe, I have supplied several parts that were omitted, & this was necessary to promise to our enquiry of the

## (Proximate Cause).

The proximate Cause has been supposed to be the first cause of fever, but I define it to be any general state of the body or of a particular part on which more immediately the symptoms depend, & which being removed the disease is removed. As an example of this, I shall consider the case of Dropsy, which I find to be a swelling in the lower belly; I examine it & see whether the swelling depends upon Air or is a Tympanites, or an water which is an Ascites. I go further than this & investigate the cause of the effusion which I find to be a Schirrhosity in the Liver — wherein this Schirrhosity consists I can't pretend to explain, but yet I have proceeded a step to the Method of Cure. In fevers I allowed that a principal part of the proximate Cause is a debility of the Nerves & power, but the exact state of the brain & the

# Recapitulation

124

Nervous fluid I am ignorant of; the fact however is sufficient to direct & influence my practice.

In Fever, notwithstanding the variety & complication of symptoms, all the phenomena depend on a simple cause, & Boerhaave & Sydenham who have adopted the Idea of a complicated cause have been embarrassed by innumerable difficulties. This then must be our first attempt, & has introduced some leading circumstances in fevers on which all the others depend. We found the cold fit to <sup>be</sup> the leading & principal symptom, & the first in the order of time, with Boerhaave we agree as to it's occurrence some intermittent; but extend it with Hoffmann in omni Febre.

Our leading proposition was, that Fever was an affection of the Nervous System, & diseases, must be generally referred to an affection of the moving powers, or the Nervous System. This though generally denied I demonstrated to be the principle Physicians proceeded on. The cold fit is an affection of the Nervous System of the Impetuum faciens according to Hoffmann, but

# Recapitulation

185

but wherein does this affection consist? we can perceive by the phenomena universally preceding fever that a debilitating cause first operates; as to the mode of its operation on the Nervous power I am ignorant & not satisfied with the fact. Next in the order of succession a spasm or constriction of the extreme vessels occurs, which is equally universal in fevers. These states are followed by an increased heat & impetus of the blood in which fever more particularly consists, & this is dependent on the other two as its proximate cause. I have mentioned in order the states of Debility, Spasm, & Reaction. For the connection of these with each other I referred you to a law of the Animal Economy the AUTOGASTRO, this I could wish still further to prosecute, & if possible explain its mechanical connection. We showed that debility might give rise to a spasm, but why the intervention of this is so absolutely necessary to produce a reaction I can not determine. This is illustrated by the operation of cold which first produces Spasm & afterwards a reaction; but a difficulty arises from the instance

\* When the action of any particular part is excited we are not to impute it to direct stimuli applied, for it often depends on causes acting in distant parts. In the cases of communication of pain from one part to another we are not to look for it from the special connection of those parts, for no connection is to be anatomically discovered; such communications must depend on the intervention of the brain & the particular state induced there, with which a particular function or organ is connected.



# Recapitulation

126.

dances of cold, and we might suppose debility to be a superfluous primary cause, & nothing is necessary but a direct cause of Spasm, as in Phlegmasia, which may produce the debility, & hence the fever; but of any such direct cause of Spasm we are entirely ignorant, & even in Phlegmasia a previous congestion occurs. In the action of cold a previous sensation of it induces a state of Debility, & hence it is probable that this is the foundation of the whole. Debility must precede Spasm & it operates without the concurrence of cold or any direct cause of Spasm, & this is always the case in a Typhoid arising without topical affection. The rigor, horror & tremor I said were symptoms of the reaction of the System, & in proportion to these the increased action is more considerable & effectual in dissipating the Spasm.

I next considered those symptoms relative to the state of the Stomach. The debility that particularly affects that Organ produces vomiting, want of Appetite, sickness, & nausea, & these are more considerable in proportion to the debility that prevails. There is another operation however

# Recapitulation

187

However, with respect to the Stomach the Nausea & Vomiting are strictly connected with the Spasm on the surface, & often proceed from this cause; from this we are led to an important doctrine in Pathology, the doctrine of Sympathetic affections. As you will perceive that the Vomiting depending on debility must be referred to a general law of the System, that Collapse or Debility produces reaction. This is established by many facts of which we have treated at large in our Institutions. The Symptoms of the Stomach precedes to the action of vomiting, Nausea &c may be considered of pernicious tendency, in so far as they express the degree of their cause, but considered as marks of a great reaction they are favourable. Vomiting is always a concomitant Symptom in Fever, & generally attended with an evacuation of Bile. This has been esteemed not only as the cause of vomiting but of fevers in general, & the opinion has subsisted for 2000 years from the time of Hippocrates to Senae, but in the autumnal season when the Bile is abundant we frequently observe its evacuation <sup>unattended</sup> with Symptoms of Fever, & wherever it occurs

# Recapitulation.

128

occurs with fever, from an abundance or acrimony of the Bile we have referred it <sup>to</sup> two causes.

1. That Intermittents & Remittents are often attended with congestions in the Vena Portarum, & hence it may be more copiously secreted & excreted.

2. In the Action of vomiting the Viscera undergo considerable pressure which emerges the biliary ducts & occasions the unusual appearance of Bile; hence it is to be esteemed as an effect & not as a cause of Fever.

It has been alleged that Contagion or morbid effluvia mixing with the bile produced the disease, but this is unequal to the production of Fever without the cause of debility combined.

I next proceed to the consideration of the Intellectual Functions, & my conclusions relative to this are founded on my physiological doctrine, 1. That intermittents in general depend on the state (or the state) of the Nervous System, & 2. the state of circulation affects these functions only in consequence of the changes produced on the former, & causes may change the state of the Nervous System by acting directly upon it.

3. 134

# Recapitulation

189

3. By the action of such causes the Nervous system is in different degrees of vigour, & the energy <sup>of the</sup> brain is in different degrees of force. These states we call Excitement & Collapse. Coherence & consistency in thinking depend on the equality of Excitement in the Nervous system, & particularly of the conarium, & on the free communication between its several parts. Delirium on the contrary depends on an inequality of Excitement in the different parts of the conarium; as proofs of this I adduced the phenomena of Sleep & Waking from the internal shut occurs between the change of the one to the other. A state of Collapse takes place in one part of the brain sooner than another, & in waking the excitement takes place by degrees. In both these states a delirium occurs, & we may observe the gradual transition wherever the cause of Excitement is applied. The delirium of fevers is easily explicable on the same footing. The cause of fever diminishes the Energy of the Brain & produces Collapse to a certain degree which often proceeds to Coma, the increased action of the Heart may also be considered as



# Recapitulation

195

a cause of excitement, & in consequence of this combination Delirium comes on.

Having thus explained delirium as depending on Debility or Collapse we may distinguish it into two kinds.

1. Where the state of Collapse is moderate & the excitement considerable, & where there is an increased impetus in the brain, then the delirium is of the phrenitic kind.

2. Where the Collapse is considerable & the causes of Excitement are applied, a Delirium occurs analogous to that occasioned in the sudden transition from Sleep to Waking, & what Pathologists have called, with propriety, *Typhomania*. This explanation of delirium illustrates our general doctrine of the subsistence of Debility in fevers. The Coma has been referred to a change in the circulating Organs, but if we consider how often it is of a transitory nature, & that the parts on dissection are in a sound condition we shall find it to be an affection of the Nervous System depending on greater Collapse & less irritation. But it is ~~not~~ not my intention here to conclude another cause for

# Recapitulation

191.

for delirium & Coma may depend on a topical inflammation of the Brain & as far as affections of the circulation.

Hitherto I have considered fevers in the Abstract, I shall now proceed to a particular investigation of their different states. To this purpose I observe that the 3 causes enumerated are constantly present together except at the beginning & end of a paroxysm, at which time they are succeeded. If this is ascertained the difference of fevers will depend on the degree & proportion of these. My first plan I considered Intermittents & continued, & found that the common distinction of these was by no means satisfactory. Some it was supposed consisted of a number of repeated paroxysms, others that subsisted for several days in one uniform paroxysm; this however is contrary to fact, for I observed that 99 fevers in 100 are such where paroxysms are repeated, & in consequence of the diurnal revolution every fever has a remission in 24 hours. The only difference is that in some the paroxysms are more obvious, & the chief difference in fevers depends on the frequent recurrence & the duration of the paroxysm.

them. We distinguish them Intermittent & Continued Fevers by the frequency of recurrence & the duration of the Paroxysm, & it is a fact that one of these states always depends on the other, viz, the frequency on the duration - an explanation of the cause of the latter will be sufficient for both.

When a person in health is engaged in Exercise, the action of the heart is increased, & the blood determined to the surface; if this is continued the sweat flows; by this the system is relaxed, but on the relaxation of the vessel the action is discontinued. In fever an excitement of the heart prevails without sweat, this frequently recurs, & it is natural to suppose some resistance takes place, for the spasm in the extreme vessels subsiding keeps up the irritation & reaction. The duration then of the paroxysm depends on the proportion of increased action, & the resisting spasm on the surface. If these were always equal we should have every fever of equal duration; but they differ with respect to duration, the difference in which must depend on the spasm being in greater proportion to the increased action, or the reaction not being sufficiently excited to overcome the spasm. This is our general doctrine which we applied to particular & mentioned two cases. Where

the Spasm is in excess or where the reaction is insufficient to dissolve it, we said that an obstinate Spasm would particularly take place, when in combination with the Diathesis Phlogistica. We generally see this in continued fevers, & Intermittents are changed to continued by causes that induce a Diathesis Phlogistica.

A 2<sup>d</sup> case is the Spasm being given, but the reaction is not duly excited, & the Paroxysm is of long duration, such are the continued fevers arising from a state of debility. We reduce then the causes of the Duration of Paroxysms,

1. To an excess of Spasm arising to phlogistic Diathesis.

2. To a debility arising from the first cause of fever.

Intermittents by Reaction & other great evacuations are changed into continued fevers from the debility induced.

This enables us more particularly to explain the distinction of continued fevers which are reduced to two cases.

1. Where Inflammatory Diathesis prevails, which gives the Inflammatory Fever.

2. Where Debility prevails & forms the Nervous Fever. The symptoms of the former show Diathesis, & the latter Debility. The only difference is that tho' I have described the



the concurrence of symptoms in each of these fevers as distinct & separate, yet they are frequently intermixed to produce a fever participating of the nature of both. The two states however never appear at the same period of the disease, but degenerate from the one to the other; thus, in the beginning, an Inflammatory state will prevail, & a Nervous towards the end. I endeavoured to show that this depended on a change in the action of the causes during the disease. When the ordinary cause of fever produces Debility, with a concurrence of an Inflammatory state, the latter will appear in the beginning when the System is vigorous; but the debilitating cause acquiring new power, & the System being weakened by the repetition of Paroxysms, the Inflammatory state is removed, & the fever is continued in the Nervous form. Thus then we have three Genera of continued fevers, The Synocha or pure Inflammatory, the Typhus or pure Nervous fever, & the Synochus or Combination of both. Our Doctrine likewise gives the difference in Intermittent Fevers, the varieties of which may be different types of the Tertian & Quartan.

A more important enquiry is concerning the cause of their change into continued — of this there are two cases.

1. Where the fever begins as a continued & changes to an Intermittent.

2. Where the fever is at first Intermittent & is changed to a continued.

The first is to be explained on a n Inflammatory state concurring at first with the causes of the Intermittent, which when diminished leaves the fever to its proper revolution & type. — as in the Typhus the inflammatory state is changed to the nervous, so the change of the intermittent to the continued may depend on the accessory cause. Phlogistic Dietesis — which may be induced from bad practice, the administration of Stimulants &c; but the most frequent cause is this, that the cause of Fever multiplies & increases its power, which proving a stronger cause of debility & weakening the reaction changes the Intermittent to the Typhus.

Another modification of Fever occurs. I have supposed that the cause of Fever acting on the Nervous System, independent of any operation on our fluids, may produce all the varieties we have  
ment

# Recapitulation

196

mentioned, but in some cases the cause of fever while it operates on the nervous system, effects a change on the common mass of blood, & this gives the peculiar modification I am to speak of. It may act like a ferment on our fluids to produce a state of putrescency or putrid fever. I mean only putridity in some degree, for absolute putrescence is incompatible with the subsistence of life.

The distinction of fevers into putrid & non putrid has been always observed, but without any precision; I confine it to a putrescency in our fluids, the marks of which I have already delivered. on this subject two cases present themselves.

1. The Introduction of a putrid ferment.
2. The perpetual disposition to putrescency, which constantly takes place in the economy even in health, which in fever from the velocity of the blood & the tone of the vessels with action on the nervous system, may produce a state of putrescency or putrid fever without the Introduction of ferments. Putrid matter in our body proves a powerful & deleterious addition, & hence the cause of fever I suppose more or less to partake of it.

As a ferment putrescency may go on to be multiplied

plied in our fluids, & not only increase in quantity but may likewise be heightened in quality, & a disease that first came on with a strong reaction may by the increased power of the putrid ferment be changed from an Inflammatory to a Nervous state.

These are not the only distinction of Fevers, for a complication may happen with the other Orders of the Pyrexia, Hemorrhage, Pleurisy, &c. of these I have given marks to determine which is the Ideopathic disease.

The difference of fever has been chiefly deduced from our general theories. We shall next see how these are supported by the phenomena that occur in the Genus & Species.

## Methodical Nosology.

The first class, in the Synopsis I have given, is not mentioned by any other Systematic. — *Febris & Pyrexia* are synonymous, with *Augures*. *Linnaeus* has comprehended the three principal orders of the *Pyrexia*, under the title of *Morbi Febriles*, in his *Clavis Classium*; his character of these (*ex sanguine in medullam*) proceeds from a bad Theory, which affords



## Methodical Nosology 198

affords a sufficient reason for our rejecting it. He like Sauvages affixes the same meaning to *Febres* that I do to *Pyrexia*; for his *Leanthemalici* &c they are merely to be considered as *Pyrexiae* that have the term *Febres* understood.

Boerhaave is the only systematic that has formed such a class as ours. This he calls *Febres*; his definition however is curious & founded on the ancient doctrine of preternatural heat. The peculiarity in my class is that I have united *Hæmorrhagiae* & *Profluvia* with *Pyrexia*. Dr Hoffmann has followed the same system, & in his *Genealogia morborum* has marked them under the same head.

We shall next mention our several Orders.

The first order of *Febres* is nearly the same with the *Sauvages*; only I thought it necessary to their further distinction to add *sinus morbo locali primario*. The *Febres* I divide into *Intermittents* & *Continued*. & hence arises a considerable difficulty in distinguishing *Remittents* from *Continued*; we observed that they differed in the frequency & duration of their paroxysms, but where we shall fix the bounds is difficult to determine. Our distinction of these

these rests upon the word *Notabili*, which signifies that we can only distinguish them by the more or less evident state of Remission. Vogel supposes that these Fevers may be comprehended under the *continua*, & his *Quotidiana continua* is a Remittent of other Physicians; we suppose that they should be united with Intermittents rather than continued for several seasons. For nothing is more frequent than for Intermittents to begin like Remittents, & assume their own form after some time. *vid. Sydenham. C. 44.*

True Intermittents often change into Remittents, of the latter are innumerable instances; a difficulty however occurs that if we take in Remittents no distinction is left for continued, but fundamentally they may all be the same. As it is difficult precisely to ascertain the limits of Remittent from continued, we therefore arrange them with those to which they have the greatest connection.

### Genera of Fever

I admit only three genera of Intermittents, the Tertian, Quartan, & Quotidian; these we have mentioned with Hoffman & Senac in an order different from the common, because I would place the most universal first. Senac rejects *quotidianas*, and as to  
(th.)

the Seclana, Septana, octana, &c. he says they are obscure & have been very rarely observed, & when they are so they are to be looked upon as casual & fortuitous, as no body has given an account of their continuance. He would not however think positively with respect to the operations of nature, for the course of nature is often so obscure that some things appear to be deviations that are in reality agreeable to it. He observes however that a paroxysm of a tertian or quartan may be suspended, & hence appear a Seclana & Octana, but when it occurs it is only an anomaly in the ordinary Intermittente.

Senac acknowledges a Quinlan, but as the quartan & tertian are so very universal it is to be presumed they are determined to this course by a law of our system, & when any deviation occurs, such Anomalys are to be considered as types or varieties of the usual forms, & not as distinct genera. These may be further accounted for by the paroxysms of the tertian & quartan being suspended for sometime; when the fever however recurs, it will return at the same period it would have done had no interruption ensued, & hence a  
tertian

tertian may form a septiana, octana, according to the number of days the attack was suspended.

In our character we have hinted at this by saying *intermittens quadraginta octo circiter horarum*, that is to say, to make allowance for the anticipation & protraction of paroxysms; for the Tertian, for instance, is not precisely periodical for 48 hours, & the case of the quintan mentioned by Van Swieten arose from a protracted Quartan, whence these are only varieties of the common & fundamental forms.

Vogel has gone to excess in bringing in his *Menstrua*, who evidently mistakes the nature of Intermittents. He supposes when an Intermittent is produced that the original cause only acts in the first paroxysm, but that the succeeding ones arise from the nature of fever in general, & the cause is not properly repealed in producing the after fits; thus was Van Swieten's Girl who had a recurrence from being frightened with a mouse, & here certainly there was nothing more than that, takes as he calls it, which former paroxysms had induced. Vogel on the other hand has explained his *Menstrua febris* on a congestion of blood in the hemorrhoids or uterus, where a any form of

Menstrua



Kinds more or less of Fever occurs; but this is in consequence of the plethoric state of the uterus on which it depends, and when the causes are removed the fever will be brought back which is quite opposite to the nature of an Intermittent. Sauvages has more properly introduced this under the title of *Ephemera Menstrua*.

Ballonius had a fever every 3 months, & some have been regularly attacked every year, in these cases there is no such connection, as between the repeated paroxysms of Intermittents that depend upon a particular recurring cause. We proceed to enquire if the genera of Tertian & Quartan are truly distinguished, for they are frequently intermixed either in the same individual or as an Epidemic, so that Tertians in July often change to Quartans in September. More than all our reasoning for the combination of Remittents with Intermittents confirms the union of Tertians and Quartans.

Dr Sydenham speaks too strongly on this subject; he says the genera of diseases are  
as

as distinct as those of plants; and after enumerating some of the principal appearances of Quartans, he says that they are as absolutely distinct from Tertians as any two plants; this however is not so much founded in observation as upon a theory of specific differences in the causes of diseases, but such specific nature is not applicable to one of 100 genera; for causes are so complicated that in many cases we are not enabled to make a distinction of the accessory from the primary cause; nor is this specific nature of the causes tolerably well founded; it may take place in some orders, as the Leanthemalici and Profluvia, but the varieties of contagion producing fever are not so considerable as Sydenham imagined.

In diseases there is nothing so fixed and properly ascertained as in the case of living Systems, or the Animal and Vegetable Kingdoms. In these the Species are absolutely fixed, and never change into one another; it has been alleged that Oats have changed into Rye, or Wheat into Beans, but this is extremely dubious. Temporary mixtures may  
indeed

# Genera of Fever

204

indeed take place, but nature soon returns to her ordinary course.

The difficulty of accurately fixing the species of diseases, renders a methodical nosology less advantageous than a regular arrangement in Botany or Zoology &c. The varieties I have mentioned and the frequent intermixture of diseases must create considerable confusion; but these are by no means general, for in most diseases there is a concurrence and combination of symptoms that are steady & not liable to change, at least to considerable varieties. These occur much more frequently than those that are irregular, and such we may safely establish as Genera.

Fever and Apoplexy are sometimes combined, but we can 999 times out of 1000 always distinguish the Idiopathic disease, & altho' Tertians & Quartans are not so easily distinguished as plants, yet they are commonly so different as to be the objects of a different prognosis and a different method of cure. This objection will equally apply to the inanimate parts of nature.

nature, the fossil kingdom &c; where the limits are not precisely ascertained, and the differences are more imperceptible. But though red and blue may be blended together into a purple so intimately that the original constituent colours shall not be distinguish-  
ed, yet we are convinced that they are se-  
parate and distinct. The Union in diseases is analogous to this, where they are so com-  
plicated and intimately blended that it is  
difficult to discover which is the Scaph-  
thic disease, but, to carry on the analogy,  
there are few purples in which we cannot  
discover one or other of the colours of which  
it is composed.

Des Haen, in the 9<sup>th</sup> part of his *Ratio Me-  
dendi*, gives a number of curious cases, where  
dissection had discovered circumstances or  
causes of diseases, which were not apprehend-  
ed from the external symptoms. (Vide. De  
Haen) — and he says, from the uncertainty of  
our diagnosis, and from the variety occurring  
in diseases a Methodic Nosology is impossible,  
but



but by a parity of reasoning we should also give up the attempt to distinguish diseases from each other. The principal question on this subject is, whether the method of description or definition should be followed in Nosology. The method of definition has been liable to abuse, but if we were to attempt description in genera we should be involved in confusion; the latter seems only only applicable to species.

The difficulty of arranging Fossils is equal to that of diseases, and the species of both are with difficulty ascertained. The Mountain crystal is found in a crude mass of the figure of an Hexagonal Prism - the variety in the length of the Pyramids, the proportion of the Bases to their Apices, the irregularity of their position, colour, &c. are so numerous that it is difficult to ascertain them by characteristic marks - Dr Hille however, who always attempts to make a large work, has made this the foundation of separate species and affixed Greek names to each; his labours

labours have accordingly proved unfortunate, as no one has followed him.

The Botanists having taken the lead in Arrangement, have applied their method to diseases and fevers, which are not such proper subjects for their particular mode of Classification, as Animals and Vegetables. Mr Cronstedt proceeds in Mineralogy to Classes, Orders, and Genera, but below this there is no regular uniform Subdivision, and he accordingly has nowhere attempted the distinction of species. It is the same in diseases; what we call Genera are often truly species, and many subdivisions must be made before we arrive at the ultimate species; we must be guided by discretion as to the extent of our subdivisions, & merely separate and unite as diseases differ or have symptoms in common.

I shall now consider the Species of Murrage's.

These Species I have arranged under the title of the several genera in our Synopsis, & I have made two different divisions of the Genera in tertian Intermittents. Interposita (Pyrrosia, & Interposita Promissione tantum. - Physicians have formed the chief distinction between Intermittent & continued fevers, from the occurrence of the former with horror, tremor, &c, many however are proper Intermittents that are unattended with those Symptoms. The first species of the Tertian in Murrage's is the Tertian legitima. I have thrown these species into 4 Fasciculi

1. Consists of such as differ in the form of their type or in the Intermision of their paroxysm.

2. Into such whose type being given are distinguished by peculiar Symptoms.

3. Whose type being given, yet the disease is distinguished by the modification of causes.

A. Such as are distinguished by a complication of other diseases, naturally & entirely independent of them.

The tertiana legitima is the fundamental form & prototype of the Tertian Intermittent. When it re-  
turns

turns regularly every other day & the paroxysm does not exceed 12 hours, it is called the *Tertiana vera, exquisita, legitima*.

The next species is the *Tertiana Duplex*. This has a paroxysm every day, but those occurring on the 1<sup>st</sup> & 3<sup>d</sup> day are similar, those on the 2<sup>d</sup> & 4<sup>th</sup> day are also similar to each other, but different from the 1<sup>st</sup> & 3<sup>d</sup>. Though this species has a paroxysm every day, yet it is known to be a tertian from the correspondence of the paroxysms on the 1<sup>st</sup> & 3<sup>d</sup> and 2<sup>d</sup> & 4<sup>th</sup> days. This being only distinguished from the Tertian by the inequality of the Paroxysm & the difference in the hour of attack, Physicians have imagined that it would scarce admit of a distinction, whence objected to its being made a different species. The improperly therefore of trogl in forming a distinct Genus of it must be evident, & still more of Linnaeus who separates the genera of double tertians and quotidianians.

III<sup>d</sup> Species, *Tertiana duplicata*. This is a Tertian where two Paroxysms happen on the alternate days, i. e. two on the odd, but on the even days none.



IV.<sup>th</sup> *Tertiana triplex*. This has two fits on the alternate days, but in the mean, or 2<sup>d</sup> & 4.<sup>th</sup> days, only one.

V.<sup>th</sup> VI.<sup>th</sup> *Amphimerina*, *Hemilriticus*, & *Pseudo Hemilriticus* are frequently remittents, but they generally have intermissions, i.e. *Asyphxia interposita*. The distinction indeed between Remittents & Intermittents is by no means exactly ascertained, as a perfect *Asyphxia* is perhaps in no case to be found. Various accounts have been given of these species, & Lehmann who has discussed the subject pretty well, mentions two *Hemilritians*.

1. Described by Hoffman, where the paroxysm occurs every day, but a double fit every other day, & this is the *tertiana triplex* of Sauvages.
2. Described by Celsus, is not so distinctly marked as to the number of fits; but a fever occurs every day, & the fit of the even day is joined to that of the odd, that is, the fit of the 2<sup>d</sup> day joins itself to the 3<sup>d</sup>; the remission in the former being almost imperceptible. Physicians have observed that when the *tertiana legitima* turned to a remittent form the paroxysms become repeat-

ed, and assumed the forms of a duplex duplicata, triplex & hemitriticus, which are all one species & only varieties of the irregular tertians.

We must observe that particular fevers come on at particular times of the day, & every symptom of fever is more especially increased towards the evening, agreeable to the diurnal habit that regulates our systems.

The Quartan generally attacks in the afternoon from 4 to 5.

The Quotidian generally in the evening.

The Tertian in the forenoon.

And when the Intermittents are changed into continued they have an exacerbation twice a day.

All paroxysms of Tertians attack about noon; but if a perfect solution does not occur, they come on again towards the evening, & become duplicata's, but without horror, &c.

The double tertian is similar to this, for whilst the fever on the odd day sufficiently denotes it to be of the Tertian kind, the double paroxysm that occurs comes on only in the evening of the even day. These different forms however cannot give different species, & only amount to our distinction

tion of regular & irregular, or legitima & illegitima.

The *Semi-quiniana*, a species of the *Amphimerina* of Sauvages, is a solitary fact, & very seldom takes place; and, when it does, is to be referred to the irregular tertians.

The *Spuria* & *Subcontinua* are truly the forms that tertians assume when changing to continued for the *Spuria* is a fit extended about 12 hours, & the *Subcontinua* to about 20 hours or more.

*S. Liperia* is mentioned by Sauvages from *Alcarenghi* as a peculiar species, but it does not differ from the irregular tertians that occur in Spain, Italy, & other warm climates. *Alcarenghi* has given us the best description of the Nervous fever, but on examining his account of the *Liperia* I find it to be the same described by *Mercatus*, *Torti*, &c.

## 2. Fasciculus. Difference of Symptoms.

*T. Potechialis*. These may readily arise from the long Paroxysms of Tertians, & the prostration a necessary attendant on protracted paroxysms & increased heat, may readily produce *Potechia*.

*T. Emetica*. As vomiting accompanies every Tertian

(c) Thus the lungs; Asthma, Affections of the organs of  
voluntary motion, Epilepsy, &c, and Scurvy might  
have added what has been marked the Hypochondria.



than it cannot be a ground of distinction except in degree, but we know *maius & minus non variant speciem*. There may be a Tertian where from a topical affection of the stomach a peculiar vomiting may take place & distinguish the species, but Sauvages has not so limited it.

*T. Carotica*. Symptoms in common we know scarcely afford a distinction to genera & much less to species, the Coma of Intermittents has been observed in all ages, but especially in those called Malignant Tertians where in a few paroxysms the fever is changed to a continued & often proves fatal.

*T. Hemiplegia*, differs in nothing from the *Carotica* which has been called *Lethargica Apoplectica Soporosa*, & other names expressing the different states of the symptoms.

*Tertiana Asthmatica Hysterica Epileptica* the meaning of which is, that while fevers are affections of the Nervous System, tho' chiefly confined to the Sanguiferous System they may be extended to different organs, & give an appearance of the several disorders to which those functions, & the particular persons are predisposed.

Senac. mentions a Lady whose tertians came on with

with great loquacity; this must certainly have been a peculiar Idiosyncrasy in the patient & not a change in the cause of fever.

Dr Casimir Medicus has given an account of Intermittents, attended with Spasm arising even to Tetanus & often Apoplexy. This may indeed affect our prognosis but can have little influence on our practice; nor does it appear that such cases were frequent even in Epidemics.

*S. miliaris & urticata*. It is difficult to say what Sauvages means by Miliaris, for he makes a false reference. I can imagine that sweat in particular persons may give a Miliary Eruption, but this does not change the species, & is Symptomatic Symptomatis rather than a Symptoma Morbi.

*S. urticata* - mentioned also by Lehmann as a Symptom frequently occurring, but it only expresses a degree of the disease.

*S. Pleuritica*. Lehmann after mentioning the varieties of Intermittents, says that they attacked often with fixed pains that personated Hepatitis, Phrenitis, Rheumatismus, Lumbago - but these are only symptoms of the cause of the disease, & not

(a) Epidemic Contagion is insufficient to produce this  
without assisted by some excess in the non natural,  
weak habit of body &c.

not of a topical affection of particular parts, & hence, are not to be distinguished as species. — The Spasm appears from Aeghorn not to be perfectly solved, & hence, it's occurring great at first may affect any of these organs disposed to Spasm; it would have been better to have united them under a species phlogistia.

When Epidemic Contagion produces Fever in warm seasons without phlogistic Diathesis, then if the phlogistic pains appear they depend on the causes of fever. But when they are protracted to the sitting on of winter, the accession of cold produces the Inflammatory Diathesis, & Phlogistic pains accompany the fever, hence Catarrhs &c. Such however more frequently accompany the vernal than the Autumnal Intermittents.

I. Arthritica. This he gets from Baimund; it is only a case of the former differing in the seat of the pain.

3.<sup>d</sup> Fasciculus, relates to Tertians that have different modifications from the state of their cause.

I. Accidentalis, where the cause is not sufficient, & some occasional circumstance co-operates. This only differs in degree from the regular tertian, it



\* Intermittents arising from the drying up of ulcers.

it is easily cured as proceeding from a weak cause.

*S. Scorbatica.* This is uncertain, as the Authors referred to knew little of the disease, the nature of Scurvy in those days not being sufficiently ascertained. Ettmüller indeed mentions it's being often combined with Intermittents.

*S. a. Scabie.* Sinker & Neutor only mention this among the the remote causes, but do not pretend to explain it.

*S. Syphilitica.* This is a complication rather than a real connection between the Sues & Intermittent, it is merely accidental, & therefore there appears to be no foundation for Sauvages practice of attacking the Sues in order to cure the Intermittent. If Mercury did cure it I would consider it as a mere fact that Mercury may sometimes cure Intermittents as well as Sues.

*N. Fasciulus,* or a combination with other disorders.

*S. Verminosa.* This occurs in many Epidemic Intermittents, & may serve to aggravate the symptoms. A late Author, M. Vander Bosch, has given an account of a *Constitutio Verminosa*, where  
he

he has referred fever entirely to Worms, not only that they aggravate the symptoms, but that the disease depends, & is modified by the presence of these worms; his chief Argument is that the fever was cured by Vermifuges; but in this view the small activity & the success of the Remedies he used, seem to show a mistake.

## Remittent Tertians.

### *Tritæophya Deceptiva.*

I have not divided the Remittent Species into Fasciculi, as formerly, tho' they are nearly arranged in the same order.

Sauvages gives a character of the Deceptiva from which no firm distinction can be made, but what is common to irregular tertians - his marks are,

1. That the patient has a sense of cold, while he is warm to the touch of a bystander.
2. Because the paroxysm of the 2<sup>d</sup> day is milder than that of the first.
3. Intermision is more & more obscured.

4. The coming on of grave quæcunque symptoms. Upon consulting his authorities for this, I find  
it

it is one of the malignant tertians & therefore  
no species.

2. Causes. On the revival of Letters men were  
well employed in investigating what was  
known by the ancients, & in more enlightened  
times Physicians have not strove less to explain  
every thing the ancients advanced, & to model  
themselves by their originals. The Ancients how-  
ever are indistinct in their notions of the types  
of fevers, & hence their notion of causes has been  
misunderstood - it is an irregular tertian, con-  
nected with the *Amphimerina Hungarica* *Calu-  
dinosa* & *Piliacea*.

In all the temperate climates when Marshy  
effluvia are raised by heat, they form the fevers  
produced by their effect is that of pure tertians.  
These effluvia are more or less poisonous in dif-  
ferent states, this depends on the nature of the  
matter, but especially on the action of heat &  
it's degree, hence in the progress of the disease  
the cause has gathered so much strength as by  
the 2<sup>d</sup> or 3<sup>d</sup> fit to obliterate the pure tertian form,  
& generally brings on some symptom, which  
if it does not kill the patient becomes purely  
remittent



remittent, and at last perhaps the disease may return to its tertian form. The consequence is that between the first irregularity in the pure tertian to the state of Ardent fever, or Causas, there is room for some distinction; but according to no method whatever can we say what are the species & what are the varieties as there are no precise limits between them.

The different Symptoms attending these, & which form no sufficient distinction for species have given rise to different names, as Leipyria, Astrotos, Rhodes, &c. &c. which are only varieties of the irregular tertian. In this view they have been esteemed by the best writers, Dr Cullen considers no difference in them but as they affect Prognosis, Dr W. Sydenham distinctly considers them as varieties.

There appears then to me only two distinctions of Tertians that deserve to be constantly marked.

1. The Regulat. )
2. The Irregular. )

Some species of Sauvages I have not comprehended, as the Tritaophya, Bratislavensis, & Americana

Americana, but these seem to have no distinction, & Dr Pringle has found that the Remittents of the Camp, & of the Marshes of Hungary & America are all nearly the same, & Dr Lind has still further confirmed this opinion.

*Tritophycia Lactea*, is a complicated case of fever depending on profluvia - it generally comes on about 9 days after child bearing, & we shall treat of it hereafter. )

## Genus II. Quartana.

*Quartana duplex* is of two kinds. 1. It is called duplex when there is a paroxysm on the 1.<sup>st</sup> 2.<sup>d</sup> 4.<sup>th</sup> & 5.<sup>th</sup>, but none on the 3.<sup>d</sup> day, the 1.<sup>st</sup> fit corresponds to the 3.<sup>d</sup>, the 2.<sup>d</sup> to the 4.<sup>th</sup> — 2. There is another where there is no fit on the 2.<sup>d</sup> & 5.<sup>th</sup> day, but on the 3.<sup>d</sup> 4.<sup>th</sup> 6.<sup>th</sup> & 7.<sup>th</sup> there is a slight evening paroxysm, which is an exacerbation, agreeable to the diurnal habit of our System.

*Quartana triplicata* has three fits on the 1.<sup>st</sup> 3.<sup>d</sup> & 5.<sup>th</sup> day, but this is a solitary instance, and so contrary to the regular course of nature, that its existence

is somewhat doubtful.

*Amphimerina Semiquartana*. This is a quartan with a paroxysm every day. It is not different from the Remittent Quartans, or Tatarophyce, except that in the latter the Quartan period comes on with greater horror.

The 2<sup>d</sup> Fasciculus, after what we have said, requires no observation.

The 3<sup>d</sup> contains Species such Sauvages thinks are distinguished by Causes.

*Quartana Infantum*: this however is no distinct species, & only differs with respect to the age of the patient; the method of cure is the same.

*Quartana Splenetica*. Sauvages says it depends on a congestion of the Spleen; this may indeed increase the heat, but large swellings on the liver & spleen frequently remain when the disease has quite disappeared. This therefore I consider as the effect & not the cause of fever, and in Do Medici's dissections there is a strong fact in favour of our opinion. In patients who had bilious symptoms he found the liver was sound tho' the bile was somewhat changed, but the spleen was generally swelled & diseased, and this is an argument

argument to me that it chiefly depends on  
Congestion, & particularly in the spleen, which  
from its structure & situation is particularly  
disposed to it.

*Sphyilitica Scorbutica Arthritica* we expunged  
from the species of Tertians, & the same argu-  
ments will hold good here.

2. *Metastatica*. This is the case of a Quartan  
alternating with an Ophthalmia. When the Oph-  
thalmia comes on the quartan returns & vice  
versa; this Sauvages considers as a Metastasis of  
translocation of morbid matter from one part to  
another, but this is groundless; for many ail-  
ments alternate without any such translocation  
of matter. This may be an instance of Tertians &  
Quartans combined with Phlegmasia, & these have  
the effect of obliterating more & more the regular  
appearances of the Intermittent; thus we explain  
the Arthritica which often alternates with Inter-  
mittents, & like other Phlegmasia may obliterate  
the appearance without being the foundation of  
the Intermittent.

2. *Amens*. This was observed by Sydenham, but  
by no one since, & at any rate cannot be considered  
as



as a species of Quartans.

### Remittent Quartans.

*Sclartophya simplex*, does not differ from the *Quartana simplex*, except in the degree of exacerbation.

*Sclartoph. semiteriana*, a small variety of the former.

*Sclartoph. Maligna*, considered the same head  
*Sclartoph. Carotica*, analogous to the comalosa &c.  
*Sclartoph. Splenalgica* & *Heptalgica* are obsor-  
 ed for the cold fit of Quartans, & of longer contin-  
 uance than any other, & therefore may occasion  
 greater congestions in the liver & spleen, but by  
 no means a cause of fever.

The Remittent quartans are not strictly to be distinguished from the Intermittents, except as species or varieties.

*Irratica*. These I have mentioned as varieties of the Tertian & Quartan type; the *Irratica* *araga* is generally a Symptomatic fever & not an Intermittent.

Genus

### Genus III. Quotidiana.

The existence of this is denied by some, & by all supposed to be very rare. Mercurialis never saw one in 40 years practice, in a country where Intermittents are very frequent.

Sauvages only founds his distinction on the fits that happen every day being <sup>exactly</sup> alike; that the times of attack too in Quotidians are the same, & occur in the morning, whereas the fit on the even day of double tertians is slighter & comes on in the evening. The evening paroxysm too of double tertians is attended with little horror, whereas every fit of quotidiens is attended with considerable horror. There is sufficient room then for a distinction & especially upon this account that Quotidians are far less apt to run into Remittents than double tertians.

I have entirely left out the Quotidiana subcontinua, it being an irregular tertian.

Quasid. Partialis. If this Sauvages mentions no case, Van Swieten two, & the Medical Essays of Edinburgh one. This is a paroxysm of fever confined to one part of the body & not extended to the whole. I observed a partial Quotidiana where the paroxysm returned

returned every day, with cold & horror in the feet, followed by the hot fit; it extended to the legs & thighs, but never higher than the haunches, & was stopped by the bark.

This is a very curious proof of the Muscular fibres of the Arteries, that they are not simple Elastics, but are proper muscular substance, endued with a vis insita that act & reflect considerable changes in the circulation independant of the heart.

2. *Hysterica Epileptica Aphorosa*, require the same remarks as *tertians* & *quartans* distinguished by symptoms.

The *Cephalagica* is a partial quotidian. Mr Senac observes that he has frequently seen some particular symptoms return periodically, thus pains in the head which are sometimes throbbing, *Ophthalmias* too he says often recur at the periods of fits, & he explains the *pleuritica* as mentioned above. I have seen such *Ophthalmias* which as they became more permanent & considerable the fits became more irregular. Senac says that he has seen the periodic *hemicranis* return with great violence every day at noon. These facts apply to the topical action of vessels when aches &c. are really

really local fevers.

The severe thus lying under a remarkable symptom have been called *lunati*.

### Remittent Quotidians.

The *Spuala* & *Stricodes* form what has been called *Algida*, & the *Syncopalis Cardicea* & *Humensae* are varieties of the same.

*Quotidiana Catarrhalis* & *Latica* are to be added to the 2<sup>d</sup> Fasciculus of the Remittents, these are symptomatic, thus the *Catarrhalis Anginosa*, *Tussiculosa*, *variolosa*, *Arthritica*, &c. by no means come under the title of quotidians.

We have now finished the subject of Intermittents & from the whole it appears that the principal distinction to be made is into Regular & Irregular.

The first admit of no distinction important enough to influence practice; the irregular admit of many divisions as the tertiana duplex, duplicata, &c. numerous of Authors, but these are not in general well founded.

Spigelius writes expressly de febre & miltetiana, & mentions under this title almost every irregular to which tertians are liable; they have been also distinguished from particular symptoms.

Mercatus, Torti, Morton & Aitken are the best writers



writers on this subject, & these will concur in informing you that these symptoms are of little consequence to practice; I will not however assert that they may not lead to particular practice when more accurately considered.

Another distinction is that Intermittents first begin in the form of continued, but soon return to their genuine type, thus displaying, according to Sydenham, their nature & genius. At other times they begin as Intermittents & terminate in continued fevers.

In the first case, the Diathesis Phlogistica subsisting, gives the continued fever, but the Acridities cause of the fever weakening the tone of the Arteries, gives at length according to its degree a Nervous or Intermittent fever as we explained before. In the 2<sup>d</sup> case the violence & quantity of the cause increasing from the heat of the fever, gives a Nervous Continued Type.

### Continued Fevers.

There is some reason to suspect whether all these at bottom are not Intermittents, at any rate the distinction between them is difficult.

The

The first distinction is that continued fevers subsist for some days without any remarkable remission, whereas such as recur with horror or great remission are universally Intermittents.

2. Every fever that has two exacerbations every day may be <sup>reckoned</sup> continued, and this appears to be the best distinction we have.

### Genera of Continued Fevers.

Sauvages has given us five genera & Linnaeus four. There they have characterized from their duration; this however is exceptionable, for they will disorder the most distinct under the same Genus; thus Sauvages has marked the *Ephemera Medocrina*, which is certainly a species of *Typhus* under the Genus *Ephemera*.

Boerhaave is equally faulty, in comprehending almost the whole under his *Synochus*, His *Epidemia* causes *Leptotyphlus*, *Phrynos*, *Synochus* &c are ridiculous as genera, since we have found them not even to give different species of Intermittents to which they belong. The best distinction of fevers seems to have arisen in England, viz,

### Inflammatory and Nervous.

This we have adopted in our Synopsis under the

the names,

## Q Synocha & Typhus. Q

These however are not always separate, they are often united, & when this is the case the symptoms of Synocha appear first & afterwards those of Typhus - we explain it thus,

Fever depends on a sedative power prevailing in the system, & this is often a septic one. If the cause is moderate the fever produced is Intermittent, & if considerable, Typhus; but if it happens, as is frequently the case in our northern climates, that the Inflammatory diathesis subsists, the symptoms of this will appear at first when the system is less debilitated, but the sedative cause increasing, (which will especially happen from the increased heat given by the phlogistic diathesis) soon removes this, and gives the fever the form of Typhus. If the cause, as frequently happens, renders our fluids putrescent, we get the febris putrida, a species of Synochus.

## Genus IV. Synocha. Q

The Synocha & Phlogosi is a Phlegmasia, & the Menstrua & Lactea belong to the Orders Hemorrhagia & Trophica.

Profluvia. The Synocha cephalagica is merely symptomatic, & the Synocha a Scabie & Tingedacere too solitary to form species.

The Althorica is especially distinguished by its cause, it arises from cold, without contagion, but if this fact was fully established in all cases it would be a sufficient ground of distinction from Typhus which commonly arises from contagion. The Iphemera we have united under Synocha as far as could be done with propriety. Sauvages enumerates many from remote causes, which may all however be reduced to the head of cold, for among the many direct stimuli mentioned none would produce fever without topical affection, and therefore cannot belong to Pyrexia sine localis primaria.

The effects of heat, exercise, wine &c. only act by producing a Debility that gives an opportunity for the operation of cold, and this causes only a shock the system but in proportion to those preceding circumstances that rendered it more liable to such action.

Sudden fear can produce fever in concurrence with cold & contagion, but it often operates singly & alone, the nature of the fever it produces is not however



however ascertained. From the instances of Swan  
Swieten it appears to be frequently Intermittent,  
and as it is a debilitating cause we should ac-  
cept Typhus rather than Synocha.

The Crapsula & Saticly may be easily admitted for  
in ordinary digestion of the most soluble food we  
have some frequency of pulse which is often pre-  
ceded by Terror. The pulse will especially be al-  
tered if the Stomach is long loaded with indissolu-  
ble food; when this occurs alone it is seldom of  
any consequence, if it concurs with any other  
fever it only aggravates but does not change  
the Genus to which it is united.

The only true Synocha then seems to arise from  
Cold, though it is a very rare occurrence, for Cold  
generally produces some topical affection with it as  
Rheumatism, Catarrh, Peripneumony, &c. I am dis-  
posed to consider these Chilogmasia as principal  
Symptoms, and Terror we may allow to be in greater  
proportion according to the degree of topical affec-  
tion, and these may be considered as Symptoms of  
fever or as primary diseases.

Sauvages gives two other Species of Synocha, the  
Languinea & depuratoria. The first Boerhaave has  
described

described under the title of *Synocha simplex*.  
When this *Synocha* is extended beyond a certain  
time he gave it the name of *Synocha putris*, and  
when the putrescency had affected the blood he  
called it *Sanguinea*.

The *Sudatoria* which is the depuratoria of Syden-  
ham is difficult to ascertain, and as this descrip-  
tion of a species is not described by Physicians, we  
are disposed to imagine Sydenham was mistaken.

## Genus V. Typhus.

This Genus is characterized by the symptoms that  
express a state of Debility; Sauvages's primary  
character is taken from the pulse, which he has esti-  
mated too low, and therefore our addition of pleurungue  
frequency was necessary. This character however  
must be received with an exception to Comatose  
cases where the pulse is far from being frequent.

The *Typhus gastricus* is not a different species from  
the pure *Typhus* or *Peripneumonia* of Lincham.

The *Typhus carcerum* of Sir John Pringle arises  
from a contagion of Human Effluvia becoming ven-  
lent from a certain degree of heat and want of ven-  
tilation. The Malign *Miasmata* generally give  
Fulor -

Intermittents, & their varieties human *Malaria Typhi*.

I must add here that Intermittent & continued fevers are specifically distinct, because continued fevers are connected with human *Malaria*, and when the Marsh *Malaria* give continued fever it may be doubtful whether human *Malaria* do not concur.

The *Typhus Aegyptiacus* so badly described by Alpinus may be referred to the Irregular *Tertians*. The *Eterodes* & *Sudatoria* will be better discussed hereafter.

*Triticophya Typhoides* is characterized by Sauvages from appearance of the infection at the beginning, but many *Typhi* before they assume their proper form have slight vague exacerbations.

### Genus VI. *Synochus*.

*Synochus Ardens*. The idea of this was taken from the great heat, but they neglected what the ancients had observed, that this was a *Triticophya* and therefore Intermittent.

*Synochus Putrida*. This may be without contagion introduced from putrescence arising spontaneously, & hence might be called *Synocha*, but that it is generally

generally contagious I shall show hereafter.

*Synochus Variolosa Dysenteriodes.* — Sydenham observes that the Matter of these may break out in the form of *Synochus*; this however is doubtful, and has not been remarked by latter observers. In all the putrid species it may be a doubt whether Inflammatory Diathesis always concurs, or whether the cause may not act like Opium as Stimulant at first and afterwards Sedative.

The *Eclica* is a genus symptomaticum, every species mentioned by Sauvages being symptomatic. —

End of the First Volume.



# Contents

## of the 1<sup>st</sup>, 3<sup>d</sup>, & 4<sup>th</sup> Volumes. Vol. 1<sup>st</sup>

	Page		Page
History of Phisic - - - - -	1.	Method in general of - - -	65.
State of Phisic as occurring in wide ages - - - - -	2.	Methodical Knowledge - - -	319.
Natural state of Phisic - - -	2.	Proximate cause of Fever - - -	99.
A 2 <sup>d</sup> condition of Phisic - - -	4.	Respiration therein - - -	94.
Asculapius - - - - -	5.	Urine therein - - -	95.
Artificial state of Phisic - - -	6.	Distinction of Section Fever - - -	99.
Rise of Asclepiades - - -	12.	----- of the Luerian - - -	99, 100.
----- Celsus - - - - -	13.	----- a Distinction - - -	99.
----- Aretaeus Cappadocius - - -	13.	Proximate cause of Fever - - -	102.
----- Galen - - - - -	13.	----- has been	105
----- Aretaeus Cappadocius - - -	15.	imputed to direct Stimuli	
----- Chemists - - - - -	16.	----- to an int.	111.
----- Paracelsus - - - - -	17.	released velocity of the Blood	
----- Sydenham - - - - -	18.	----- to Lantier	112.
----- Stahl - - - - -	21.	and Acrimony - - -	
----- Boerhaave - - - - -	24.	----- to Spahn	113.
----- Hoffman - - - - -	26.	of the extreme Intense - - -	
System of Licetand - - -	43.	Stability however in the	119
		but cause of the cold	
		Fit of seeds - - -	

Debility lays the foundation & is followed by Spasm & increased Action -	122.
Hemorrhage -	123.
Objection to the proximate cause said down by us -	125.
Symptoms relative to the Stomach -	135.
Bile weakened to be a cause of Fever -	141.
Febrile Microcrata united with the bile weakened a cause of Fever -	145.
Febrile Delirium -	151.
Febriles -	158.
Division of Febriles into Intermittent, (continued) & Remittent -	159.
----- into Continents -	160.
Pyrexia what it depends on -	166.
Synocha or Infl. Fever -	170.

Leptococcus or Mercurius Fever -	173.
Synocha or Putrid Fever -	177.
Recapitulation -	182.
Genera of Fever -	199.
Species of Synocha -	208.

**Vol 3.** <sup>contains Phlegmasia, Sic, Exanthemata, Hemorrhagia & Influvia.</sup>

Phlegmasia Order -	1.
Phlegmon -	2.
----- terminations -	16.
1st Resolution -	16.
2nd Suppuration -	17.
3rd Gangrene -	23.
4th Schirrus -	25.
5th Effusion -	26.
Distinction between Phlegmon & Erysipelas -	32.
Cure -	34.
Ophthalmia -	50.
----- cure of -	55.
Phoritis -	62.
----- Cure -	63.
Cynanche Phlegmon -	64.
----- Cure -	66.

Croup - - - - -	69
Cynanche trachei: -	71
- - - - - Cure -	73
Pneumonic affec <sup>ns</sup> } viz. Peripneumonia } Pleuritis & peritonitis } - - - - - Terminations	74
- - - - - cure -	94
Pleuritis puris -	93
Peripneum: Vesica -	93
Peritonitis -	102
Gastritis -	105
- - - - - cure of -	107
Enteritis -	108
- - - - - cure of -	110
Hepatitis -	112
- - - - - cure of -	114
Splenitis -	115
Nephritis -	116
Cystitis -	118
Hysteritis -	118
Rheumatismus acut: -	120
- - - - - Diagnosis -	120
- - - - - Prox: cause of -	123
- - - - - cure of -	128
Dover's Powder how } to be administered }	134

Rheumatism Chron: -	137
- - - - - cure -	137
Arthritis - - - - -	139
- - - - - Proxim: cause -	140
- - - - - cure -	172
Arthritis Atomica -	202
- - - - - Cure -	203
Ord: <u>Lownthorn's</u> -	208
Impetigo - - - - -	214
- - - - - cure -	217
Psoriasis - - - - -	220
- - - - - nature of -	224
- - - - - species of -	225
- - - - - prognosis -	226
- - - - - cure -	226
- - - - - Prophylaxis of -	237
Varicella - - - - -	239
- - - - - historia morbi -	239
Confluent & distinct, how } to be distinguished - }	240
- - - - - how caused -	241
- - - - - Inoculation for -	244
- - - - - cure -	256
- - - - - Secondary fever in -	261

Varicella	264
Rubella	265
— cure of —	268
Miliaria	273
— cure of —	278
Scarlatina	281
Urticaria	283
Pemphigus	284
Aphtha	286
Ord: <u>Hæmorrhagice</u>	285
— Activa series of	
Phænomena in —	287
— cure —	294
Epistaxis	312
Hæmoptysis	314
— cure of —	322
Phthisis Pulmonalis	318
— cure of —	322
Menorrhagic	331
— cure of —	339
Leucorrhœa	343
Abortus	345

Amenorrhœa of 2 kinds	349
viz 1. Inansio Mensium	350
— cure of —	352
2. Suppression Mensium	353
Hæmorrhœis	358
— cure —	364
Ord: <u>Profluentia</u>	372
Catarrhus	374
Dysenteria	380
— cure —	386

<u>Vol 4.</u>	contains Yenc rosist & (uchex: ia
(I. <u>Neurosis</u>	1
Ord. <u>Comata</u>	5
Apoplexia	7
— Proximate cause of —	8
— cure of —	22
Paralysis	27
— Prox: cause of —	29
— cure of —	35
Catalepsia	42
Ord. <u>Adynamia</u>	43
Syncope	45
— cure of —	53



Dyspepsia	55	Setanus	161
febrilis	76	Palpitation	163
Paralytica	76	Asthma	165
Hypochondriaca	76	cure	173
Hysterica	77	Pertussis	183
Chlastica	78	cure of	186
Catamenialis	80	Pyrosis	190
Cachectica	81	Colica	195
Hemorrhoid:	81	cure of	199
Arthritica	83	Cholera	209
Nephritica	83	cure of	211
cure of	83	Diarrhea	213
Hypochondriasis	111	cure of	215
cure of	112	Diabetes	221
Chilrosis	118	cure of	228
cure of	119	Hysteria	235
Ord. Spasmi	120	remote causes	231
Epilepsia	124	Hydrophobia	240
remote causes of	125	Ord. Vesania	241
cure of	144	Amantia	244
Convulsio	156	Mania	245
Chorea Sancti Viti	157	Melancholia	256
cure	159	Somnium	261

Cl. Cachix ——— 262

Ord. Marcores ——— 265

Tubes ——— 267

Atrophix ——— 270

Ord: Intumescentia 272

Polysarcia ——— 273

Pneumatosis ——— 275

Physometra ——— 276

Tympanites ——— 276

Hydropleurix species a 278 ad 300

viz Anasarca,

Hydrocephalus

Hydrocrachitis

Hydrothorax

Ascites,

Hydrometra

Hydrocele

Physconia ——— 300

Rachitis ——— 301

——— Prox: cause 305.

Ord: Impetigines — 310

Scrophula ——— 311

Syphilis ——— 319

Scorbutus ——— 334

Scrophantiasis — 338

Sipra ——— 338

Strambasia — 338

Trichoma ——— 338

Teteru ——— 339.





















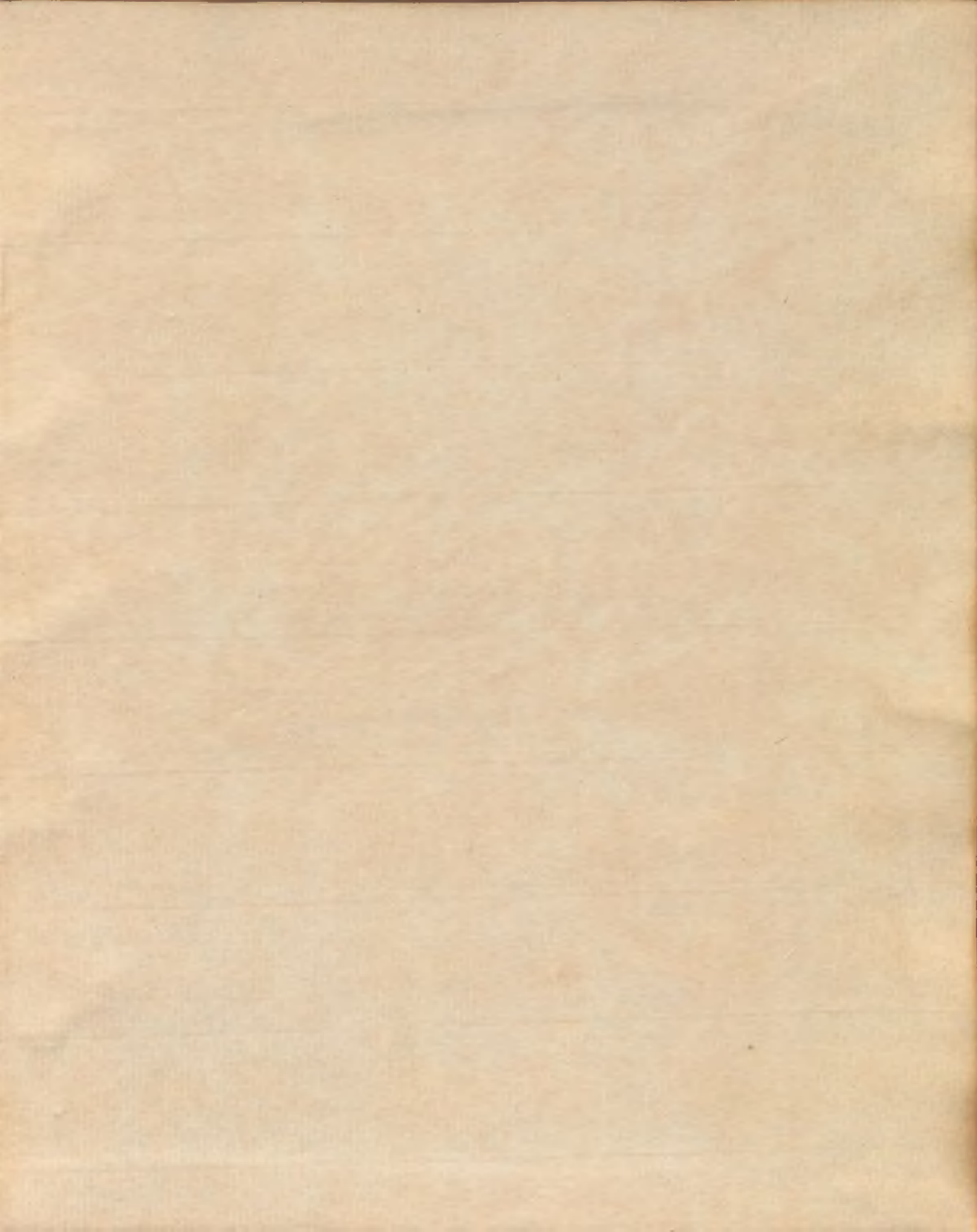












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